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OF THE

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VOL. 74 JUNE, 1948 NO. 6

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AND

DISCUSSIONS

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AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

VALUATION AND DEPRECIATION OF PUBLIC UTILITY PROPERTY

BY MAURICE R. SCHARFF,[1] M. ASCE

SYNOPSIS

This paper reviews the status of the "fair value" and depreciation problems as they have been left by decisions of the United States Supreme Court in cases arising under the Natural Gas Act (315 US 575; 320 US 591; and 324 US 636), and concludes that the confusion in which they are enmeshed is deeper than ever. It is suggested that because of this confusion, and because of the inadequacy of currently accepted theories for the adjustments that will be required if the prospects of revolutionary developments in the field of power generation are realized, a general reconsideration of these problems by the engineering profession is appropriate.

A solution is suggested based on the conceptions that "fair value" and "service value" are identical and that reasonable rates are no more than those required to cover the cost of rendering the existing service if provided by facilities of the most modern and efficient kind available as a result of progress of the arts, and of capacity no greater than that required to meet the demand. It is proposed that the equal annual payment annuity, at a reasonable rate of return, on the cost of such facilities be included in such costs; and that the "fair value" or "service value" of the existing property is the present worth at a reasonable rate of return, for a reasonable future capitalization period, of the amount available from such reasonable rates after deducting the operating expenses, other than depreciation, of the existing facilities.

If this solution were adopted, it is further proposed that the difference between the cost of reproduction new of the existing property and its "fair value" or "service value," as referred to previously, be considered as a measure of accrued depreciation; and that the ratio of such accrued depreciation to the cost of reproduction new of the existing property be taken as a measure of the

NOTE.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **November 1, 1948.**

[1] Cons. Engr., New York, N. Y.

existing percentage of accrued depreciation. This percentage could reasonably be used as a measure of the "depreciation reserve requirement" under those classifications of accounts which define depreciation as "the net loss in service value not restored by current maintenance." It is also suggested that the proposed solution would permit the reasonable annual depreciation expense for the purpose of such classifications of accounts, as previously referred to, to be determined by taking the difference between the "depreciation reserve requirement," at the beginning and the end of each year, measured in the manner previously referred to, and adding the net debit to reserve during the year on account of retirements.

An example is given which illustrates the application of the suggested solution of the problem to data assumed for a hypothetical manufactured gas plant.

Introduction

This paper is not intended as a detailed review of the past discussions on this subject in Society publications,[2,3,4,5] or of the voluminous literature that has grown up in court and commission decisions and in textbooks and periodicals. Nevertheless, these references, together with others to which they will refer the reader desiring a more complete knowledge of the literature, reflect the confusions and contradictions in which the whole subject of the valuation and depreciation of public utility property had become enmeshed prior to the decisions of the United States Supreme Court in the Natural Gas Pipeline Company Case (315 US 575),[6] the Hope Natural Gas Case (320 US 591),[7] and the Panhandle Eastern Case (324 US 636).[8] In these cases the Supreme Court upheld orders of the United States Federal Power Commission (FPC), fixing just and reasonable rates under the provisions of the Natural Gas Act on a basis which is perhaps best stated in the Natural Gas Pipeline Company Case opinion[6] as follows:

> "The Constitution does not bind rate-making bodies to the service of any single formula or combination of formulas. Agencies to whom this legislative power has been delegated are free, within the ambit of their statutory authority, to make the pragmatic adjustments which may be called for by particular circumstances. Once a fair hearing has been given, proper findings made and other statutory requirements satisfied, the courts cannot intervene in the absence of a clear showing that the limits of due process have been overstepped. If the Commission's order, as applied to the facts before it and viewed in its entirety, produces no arbitrary result, our inquiry is at an end."

---

[2] *Transactions*, ASCE, Vol. LXXXI, 1917, pp. 1311–1581.
[3] *Ibid.*, Vol. 104, 1939, pp. 1127–1138 and 1149–1209.
[4] *Ibid.*, Vol. 108, 1943, pp. 1235–1342.
[5] *Ibid.*, Vol. 111, 1946, pp. 1343–1400.
[6] *Public Utilities Reports*, New Series, Vol. 42, 1942, p. 129.
[7] *Ibid.*, Vol. 51, 1944, p. 193.
[8] *Ibid.*, Vol. 58, 1945, p. 100.

In the same case (in which, curiously enough, the FPC used a "cost of reproduction" rate base), Justices Hugo L. Black, William O. Douglas, and Frank Murphy wrote a concurring opinion in which they expressed the view that this was

> "* * * an appropriate occasion to lay the ghost of Smyth v. Ames (1898) 169 US 466, 42 L ed 819, 18 S Ct 418, which has haunted utility regulation since 1898. That is especially desirable lest the reference by the majority to 'constitutional requirements' and to 'limits of due process' be deemed to perpetuate the fallacious 'fair value' theory of rate making in the limited judicial review provided by the act. * * * As we read the opinion of the court, the Commission is now freed from the compulsion of admitting evidence on reproduction cost or of giving any weight to that element of 'fair value.' The Commission may now adopt, if it chooses, prudent investment as a rate base—the base long advocated by Mr. Justice [Louis D.] Brandeis. And for the reasons stated by Mr. Justice Brandeis in the Southwestern Bell Telephone Case there could be no constitutional objection if the Commission adhered to that formula and rejected all others."

In the Hope Natural Gas Company Case the Supreme Court upheld a rate order based upon a fair return on "actual legitimate cost" less "accrued depletion and depreciation" on an "economic service life basis," whereas the FPC had disregarded the testimony submitted in that case on reproduction cost less "observed depreciation" as of no probative value. In its opinion the Court stated:

> "When we sustained the constitutionality of the Natural Gas Act in the Natural Gas Pipeline Company Case, supra, we stated that the 'authority of Congress to regulate the prices of commodities in interstate commerce is at least as great under the Fifth Amendment as is that of the states under the Fourteenth to regulate the prices of commodities in intrastate commerce.' 315 US at p. 582, 42 PUR (NS) at p. 135. Rate making is indeed but one species of price fixing. Munn v. Illinois (1877) 94 US 113, 134, 24 L ed 77. The fixing of prices, like other applications of the police power, may reduce the value of the property which is being regulated. But the fact that the value is reduced does not mean that the regulation is invalid. Block v. Hirsh (1921) 256 US 135, 155–157, 65 L ed 865, 41 S Ct 458; Nebbia v. New York (1934) 291 US 502, 523–539, 78 L ed 940, 2 PUR (NS) 337, 54 S Ct 505, 89 ALR 1469 and cases cited. It does, however, indicate that 'fair value' is the end product of the process of rate making, not the starting point as the circuit court of appeals held. The heart of the matter is that rates cannot be made to depend upon 'fair value' when the value of the going enterprise depends on earnings under whatever rates may be anticipated.
>
> "We held in Federal Power Commission v. Natural Gas Pipeline Co., supra, that the Commission was not bound to the use of any single formula or combination of formulae in determining rates. Its rate-making function, moreover, involves the making of 'pragmatic adjustments.' Id. at p. 586. And when the Commission's order is challenged in the courts, the question is whether that order 'viewed in its entirety' meets the requirements of the act. Id. at p. 586. Under the statutory standard of 'just and reasonable' it is the result reached, not the method employed, which is controlling. Cf. Los Angeles Gas & E. Corp. v. California R. Commission, 289 US 287, 304, 305, 314, 77 L ed 1180, PUR 1933 C 229, 53 S Ct 637; West Ohio Gas Co. v. Ohio Pub. Utilities Commission (1935) 294 US 63, 70, 79 L ed 761, 6 PUR

(NS) 449, 55 S Ct 316; West v. Chesapeake & P. Teleph. Co. 295 US 662, 692, 693, 79 L ed 1640, 8 PUR (NS) 433, 55 S Ct 894 (dissenting opinion). It is not theory but the impact of the rate order which counts. If the total effect of the rate order cannot be said to be unjust and unreasonable, judicial inquiry under the act is at an end. The fact that the method employed to reach that result may contain infirmities is not then important."

In the Panhandle Eastern Case the court upheld a rate order of the FPC based on "actual legitimate cost" less straight-line depreciation, and on a refusal even to accept testimony on reproduction cost less observed depreciation. In this case, the Court stated in its opinion:

"* * * The question on review is not the method of valuation which was used but the end result obtained since the issue is whether the rate fixed is 'just and reasonable' * * *."

These expressions of the United States Supreme Court have been hailed by the FPC and by other advocates of the prudent investment or original cost principle of rate making as destroying the fair value principle and giving blanket approval to rate orders designed to yield a fair return on original cost less straight-line age-life depreciation. Examples are the opinion of the Utah Supreme Court in the Utah Power and Light Company versus Public Service Commission Case,[9] where the Court stated as follows:

"From the foregoing it becomes clear that the Supreme Court is now committed to the view that there is no constitutional requirement that utilities be permitted to earn a 'fair return' on the 'fair value' of the property devoted to the public use. The Hope Case, supra, stands squarely for the doctrine that it is the final impact of the rate order which is controlling in so far as Federal constitutional limitations are concerned. So long as the rate set does not confiscate the property devoted to public service, the rate order will not be held to violate substantive constitutional principles. The legislature is free to determine its own economic policy in regard to the fixing of rates. Its power to set rates is, however, still circumscribed by two constitutional limitations: (1) substantive constitutional law requires that the rates finally set shall not be confiscatory; and (2) the requirements of procedural due process must still be followed."

Another example of this interpretation of the decisions of the Supreme Court is the opinion of the Louisiana Public Service Commission in its proceeding relative to the rates of the Louisiana Power and Light Company,[10] from which the following sentences are quoted:

"* * * finally in 1944, the United States Supreme Court in the Hope Natural Gas Company Case laid the ghost of Smyth vs. Ames to rest and left regulatory commissions free to build the rate regulation process on 'Prudent Investment,' as far as the Federal Constitution is concerned. Commissions are left free to make pragmatic adjustments within the statutory (and judicial) ambit of their authority.

"Although this Commission has in the past followed the 'Fair Value' theory in rate making proceedings, and although this process has been approved by the Supreme Court of this State, this approval was required by the former U. S. Supreme Court jurisprudence on Federal due process.

[9] *Public Utilities Reports*, New Series, Vol. 56, 1944, p. 136.

[10] *Ibid.*, Vol. 65, 1946, pp. 18 and 22.

Since there are no Louisiana decisions requiring use of the 'Fair Value' theory in public utility rate making to meet State requirements of due process, and since there are no state statutes directing use of the 'Fair Value' theory, it is the opinion of this Commission that the Supreme Court of Louisiana is free to allow the use of the 'Prudent Investment' theory in public utility rate making by this Commission. Therefore, being desirous of simplifying the rate making process by eliminating the necessity for and the use of the cumbersome, expensive, time wasting and inconclusive reproduction appraisals and studies used in the past, we herewith adopt the 'Prudent Investment' theory of rate making for public utilities and set out below our application of this theory."

These attempted extensions of the opinions of the United States Supreme Court have not gone without contradiction by members of the Court itself, and by other competent students of the valuation problem. Mr. Justice Robert H. Jackson (with whose dissent Mr. Justice Felix Frankfurter expressed general agreement) pointed out in his opinion in the Hope Case that all that the Court had done in the earlier Pipeline Company Case had been to release the Commission:

"* * * from subservience to any single formula or combination of formulas provided its order, 'viewed in its entirety produces no arbitrary result' 315 US at p. 586."

—and he expressly disclaimed approval of the contrary view of the minority concurring opinion in the Pipeline Case, using the following language:

"The minority opinion I understood to advocate the 'prudent investment' theory as a sufficient guide in a natural gas case. The view was expressed in the court below that since this opinion was not expressly controverted it must have been approved. I disclaim this imputed approval with some particularity, because I attach importance at the very beginning of Federal regulation of the natural gas industry to approaching it as the performance of economic functions, not as the performance of legalistic rituals."

Mr. Justice Jackson also stated in his dissenting opinion in the Hope Case:

"I must admit that I possess no instinct by which to know the 'reasonable' from the 'unreasonable' in prices and must seek some conscious design for decision. The court sustains this order as reasonable, but what makes it so or what could possibly make it otherwise, I cannot learn."

Mr. Justice Stanley F. Reed, also, dissented in the Hope Case in an opinion, in which he, too, denied that the opinion of the Court approved the use of prudent investment alone in determining the rate base, adding:

"This leaves the Commission free, as I understand it, to use any available evidence for its finding of fair value, including both prudent investment and the cost of installing at the present time an efficient system for furnishing the needed utility service."

In the margin he also stated,

"'Reproduction cost' has been variously defined, but for rate-making purposes the most useful sense seems to be, the minimum amount necessary

to create at the time of the inquiry a modern plant capable of rendering equivalent service. See I. Bonbright, Valuation of Property (1937) 152. Reproduction cost as the cost of building a replica of an obsolescent plant is not of real significance."

Mr. Justice Jackson again returned to his contentions in a concurring opinion in the Colorado Interstate Gas Company Case (325 US 581),[11] in which he stated:

"I do not recede from the views therein stated that Hope provides no workable basis of judicial review, no key by which Commissions can anticipate what rule, if any, will control our review, and no guidance to counsel as to what issues they should try or how they should try them."

Robert L. Hale concludes,[12] apparently with regret, that the Natural Gas Pipeline Case left the Supreme Court still committed to what he considered the totally irreconcilable rate criteria of that famous case, "* * * the ghost of Smyth v. Ames still walks, and utility regulation continues to be enveloped in its fog."

The result has, therefore, been not clarification and clear decision, but confusion worse confounded. Consideration of this situation, after nearly 4 years of complete detachment from the subject while in military service, and review of the previous discussions in Society publications to which reference has been made (which seem to contain promise of possible real progress if they could be continued) have led the writer to propose the subject for further consideration and discussion by the engineering profession.

An additional reason why such reconsideration appears timely just now is the widespread scientific and engineering activity in the field of electrical power production. The gas turbine, the powdered fuel turbine, the application of atomic energy, either as a source of heat or as a direct source of electrical energy —any one of these may lead to a revolutionary development in this field within the next five to twenty-five years. It is, of course, not possible to predict at this stage of development that any one of them actually will reduce substantially the cost of power production. Neither is it possible to predict with assurance that no one of them will do so.

If such a development should occur, it is not to be expected that regulatory commissions will undertake to assure that existing steam and hydroelectric power plants will be permitted to earn a return even on their original cost less straight-line depreciation, without giving effect to the economies which such a development in the progress of the arts would make possible.

It is hoped that the suggested consideration and discussion by the engineering profession may make a useful contribution toward the relief of the present unhappy situation in this field, and may suggest the direction in which an answer can be sought that will appeal to judgment and conscience.

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[11] *Public Utilities Reports*, New Series, Vol. 58, 1945, p. 83.

[12] "Does the Ghost of Smyth v. Ames Still Walk?" by Robert L. Hale, *Harvard Law Review*, Vol. 55, 1942, p. 1116.

Discussion

As a basis for the suggested reconsideration by the engineering profession, it is proposed to go back to Smyth versus Ames (169 US 466) and, passing over the catalog of matters for consideration in determining fair value, which have been the source of so much discussion and confusion, to concentrate on two sentences from that often quoted opinion as follows:

> "What the Company is entitled to ask is a fair return upon the value of that which it employs for the public convenience. On the other hand, what the public is entitled to demand is that no more be exacted from it for the use of a public highway than the services rendered by it are reasonably worth."

Are these two criteria for the determination of reasonable public utility rates necessarily irreconcilable, as so many students of that opinion have asserted? Also, is "* * * the fog in which speculations based on Smyth v. Ames have enveloped the practical task of administering systems of utility regulation," as referred to by Mr. Justice Frankfurter (307 US 104, 123)[13] in Driscoll versus Edison Light and Power Company, and which was discussed in the introduction to this paper, a necessary result of the Court's decision; or has this confusion arisen in large measure from the failure of the engineering profession to provide an application of these criteria through a solution of the valuation and depreciation problems which reconcile them in accordance with an independent principle? Furthermore, is such an independent principle available for this purpose?

The attempt to apply these criteria by deriving reasonable rates from consideration of a fair return upon either reproduction cost or original cost of the existing property less observed or age-life depreciation, assumed to represent fair value is, as has so often been pointed out, reasoning in a circle. Neither do the methods of estimating depreciation, to which reference has been made, bear any known relation to the effect of obsolescence and changes in demand which are generally agreed to be the major causes of retirement of utility property.

It is suggested that the circle of reasoning, in attempting to base rates on value and value on rates, might be broken by recognizing that the fundamental purpose of public utility rate regulation is to give effect to the public policy of avoiding the waste that would result from leaving price fixing for public utility service to the ordinary forces of competition, and to subject such rates to control based upon simulating competition. As stated in one report,[14]

> "The purpose of regulatory procedure in the protection which it is designed to afford the consumer is to simulate and substitute the effects of competition and give the consumer the benefits which he would derive from a system of competition."

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[13] *Public Utilities Reports*, New Series, Vol. 28, 1939, p. 65.

[14] Report of the Committee on Progress in Public Utility Regulation, National Assn. of R. R. and Utilities Commrs., 1941, *Proceedings*, 53d Annual Convention of the National Assn. of R. R. and Utilities Commrs., Washington, D. C., p. 366.

To state the matter another way, the reasonable rates which are aimed at by rate regulation should be such as will produce a gross revenue which is no more than sufficient to pay the cost of operating expenses and fixed charges necessary to provide the service if furnished by facilities of the most modern, efficient, and economical kind available in the market as a result of progress in the arts, and of capacity adapted to the requirements of the service. The "fair value" or the "service value" of existing equipment is, then, no more than the capital sum corresponding with the present worth of the amounts expected to be available for fixed charges out of such reasonable gross revenues, after deducting the operating expenses (other than depreciation) of the existing facilities. This formula for "fair value" or "service value" can be restated, for ease of computation, as the present worth of the equal annual payment annuity on the estimated cost of the facilities required to furnish the service most economically, reduced by the excess of the operating expenses of the existing facilities over the operating expenses of the most economical facilities.

It is suggested that this approach would actually reconcile the two criteria of Smyth versus Ames and, in providing an independent principle for the determination of reasonable rates, would at the same time provide an economically sound solution of the valuation and depreciation problem. Under this solution, fair value would be the amount upon which a reasonable fixed charge for depreciation and return would remain after deducting from revenue at reasonable rates the operating expenses of the existing facilities; and accrued depreciation would be the difference between such fair value and the cost of reproduction of the existing facilities.

This solution of the problem is also in accord with the classical economic theory of price fixing in a free market, inasmuch as the equating of the depreciation and return on fair value with the value of the service, as measured by the cost of providing it most economically, corresponds with the determination of the price at which a willing buyer might reasonably puchase and a willing seller might reasonably sell a needed utility service. This approach also reconciles the "fair value" principle with the "yardstick" principle, and if applied in accordance with sound engineering judgment would make unnecessary the construction of "yardstick" plants to secure the benefit of true "yardstick" rates.

This concept is not novel. It is suggested herein that it is implied in any attempt at reconciliation of the two criteria of Smyth versus Ames; and Mr. Justice John M. Harlan, who wrote the Smyth versus Ames opinion, referred to it even more specifically in San Diego Land and Town Company versus National City Case (174 US 739,757), in which he referred to the original cost basis of valuation as "defective in not requiring the real value of the property and the fair value of the services rendered to be taken into consideration." It has been applied in the valuation of particular items of property in a number of commission cases, as follows: Michigan State Telephone Company,[15] Hill versus Antigo Water Company,[16] Somerville Water Company,[17] Wood versus

[15] *Public Utilities Reports*, Vol. 1921C, p. 545.
[16] Wisconsin R. R. Comm. Repts., Madison, Wis., 1909, Vol. 3, p. 623.
[17] *Public Utilities Reports*, Vol. 1922C, p. 309.

Elmira Water, Light and Railroad Company,[18] and Department of Public Service versus Pacific Telephone and Telegraph Company.[19]

The same thought has been discussed at length by J. C. Bonbright. To select one statement from his discussion,[20]

> "* * * it would seem * * * that, if reproduction cost is used as a basis of valuation, the only significant cost is that of constructing a modern substitute plant. The cost of producing a substantial replica would then be ignored except as an indirect means of estimating the former figure. Even the cost of substitute plant could not be used directly as the basis of valuation; for its greater efficiency would mean savings in operating expenses. The operating disadvantage of the present plant as compared to the hypothetical new plant should be estimated by a capitalization of the difference in annual expenses, which should be deducted from the cost of the new plant in arriving at the old one."

It was also directly referred to in Mr. Justice Reed's marginal note in his dissenting opinion in the Hope Case, previously quoted. However, as far as the writer knows, this approach has never been completely applied in any valuation and rate case.

Possibly the reason for failure to adopt this approach is its obvious difficulty; as Mr. Bonbright states:[21] "A very nice appraisal problem—and one which calls for great engineering, accounting, and mathematical skill." On the other hand, millions of dollars are invested every year in new construction, replacement, and improvements, based upon engineering estimates and forecasts such as are standard in engineering economics and which, as explained by Eugene L. Grant,[22] M. ASCE, are different in no way from the engineering estimates required to measure reasonable rates and fair value in the manner herein suggested.

Also, it is not believed that the adoption of this suggestion would be in conflict with the rejection by the United States Supreme Court in the Indianapolis Water Company Case (272 US 400),[23] of the inclusion of a substitute for a portion of the property in an estimate of reproduction use. Examination of that opinion shows that a witness for the city proposed to eliminate a portion of a canal and a power plant used to generate power for pumping from the water carried in the canal, and to substitute a steam plant with a lower reproduction cost. The opinion does not make clear whether the witness gave any effect to the increased operating expenses that would have resulted from the substitution of the steam plant. In any case, the opinion indicates that the proposal was merely to substitute a different engineering judgment from that which had led to the construction of the canal and power plant—which engineering

[18] *Public Utilities Reports*, Vol. 1927B, p. 400.

[19] *Ibid.*, New Series, Vol. 37, 1940, p. 321.

[20] "Valuation of Property," by J. C. Bonbright, McGraw-Hill Book Co., Inc., New York, N. Y., 1937, p. 1124.

[21] *Ibid.*, p. 164.

[22] "Relationship to Competitive Industry," by Eugene L. Grant, in "Fundamental Aspects of the Depreciation Problem: A Symposium," *Transactions*, ASCE, Vol. 108, 1943, p. 1242.

[23] *Public Utilities Reports*, Vol. 1927A, p. 15.

judgment had, as a matter of fact, been specifically approved by the Indiana Public Service Commission in two previous opinions quoted in the opinion of the Supreme Court. As the Supreme Court stated: "There is to be ascertained the value of the plant used to give the service and not the estimated cost of a different plant"; but the consideration of the effects of obsolescence and changes in demand upon the value of existing facilities, by use of estimates of cost of installation and operation of facilities of the most economical type and of capacity adapted to the requirements of the service, is a wholly different matter from substituting the estimated cost of reproduction of a different plant for that of an existing plant.

There is no compelling reason, in theory, why the method used to estimate the value and depreciation of public utility property should have any necessary relation to public utility accounting. Accounting records provide a conventional method of informing managers, investors, and public authorities regarding the financial and fiscal operations of business enterprises, and are controlled more by "generally accepted accounting principles," and by prescribed standards than by any rigid technical or economic theories. Nevertheless, most public utility regulatory authorities have prescribed uniform classifications of accounts which define depreciation as "the net loss in service value not restored by current maintenance," and provide for charging in each month "the estimated amount of depreciation accrued during that month." These provisions cannot be given any literal application, since these systems also require the recording of "costs," not "service values"—except on the basis of the economically and legally unsound circular reasoning assumption that "cost" and "service value" are identical.

However, if the regulatory commissions believe, as the New York Public Service Commission stated in its memorandum on unification plans,[24] that regulation would be facilitated if "depreciation reserves represented even approximately the amount of depreciation actually existing in the property," then a helpful meaning can be given to the language of these requirements by maintaining depreciation reserves in the accounts which are, as nearly as possible, the same proportion of the original cost of the depreciable property as the accrued depreciation in the property is of its cost of reproduction new. If this interpretation is adopted, the difference between the cost of reproduction new of the existing property and its "fair value" or "service value" can be considered as a measure of the accrued depreciation; the ratio of such accrued deprecation to the cost of reproduction new of the existing property can be considered as a measure of the percentage of accrued depreciation; and this ratio can be applied to the original cost of the depreciable property to arrive at a measure of the desirable depreciation reserve requirement for accounting purposes. Furthermore, to preserve this relationship, the depreciation reserve requirement can be determined at the beginning and end of each year, and the net difference, plus the net debit to reserve during the year on account of retirements, can be taken as a corresponding measure of annual depreciation expense for accounting purposes.

[24] Annual Report, New York Public Service Comm., Albany, N. Y., November 10, 1936, Vol. 1, p. 724.

Illustrative Example

Application of this suggestion to the determination of the reasonable gross revenue, valuation, and depreciation of a specific utility is illustrated by an example of a hypothetical manufactured gas plant, based upon data selected from a number of different manufactured gas appraisals made by the writer prior to 1942. Any resemblance of this hypothetical plant to an existing plant is purely coincidental.

In developing this example it should be pointed out that a practical solution is aimed at, and not a solution of the theoretical problem. The latter could only be solved if the gift of prophecy were available as to the quantity of service to be required, the competitive conditions under which it would have to be sold, the cost of installation of the most economical facilities for supplying the service, the operating expenses (both of this most economical installation and of the existing facilities), the mortality characteristics of each successive group of units of property, the fair rate of return, and various other factors for each year of an indefinite period in the future. Instead, the solution sought is one analogous to the solution of such problems as are encountered in the daily activities of business men, financiers, and entrepreneurs. Such solutions often seem to be reached without taking into account any quantitative considerations whatever. Sometimes the business man estimates the future earning power of a facility on the basis of past experience by application of a rule of thumb, rather than by calculation of present worths. Sometimes he has technical advice which takes into account the best possible estimates of relative costs of installation and operation of various alternatives and the present worths of prospective returns as a basis for judgment.

An estimate of this kind is illustrated by the following example: It is suggested that such a computation, based upon the present worth of the equal annual payment annuity on the estimated cost of the facilities of the most economical type available as the result of the progress in the arts and of capacity no more than is required to provide service performed by the existing facilities, less estimated average annual savings in operating expenses that could be accomplished by utilization of these most economical types of facilities, comes as close to the solution of the theoretical problem as is practically possible, and provides a practical solution that can be accepted as the most reasonable one available.

For purposes of the example, a manufactured gas plant has been assumed with $50,000,000 of actual original cost, and $60,000,000 of reproduction cost new of coke ovens and water gas sets; pumping stations and gas holders; cast iron, wrought iron, and steel mains; wrought iron and steel services; meters; general structures; and general equipment. It is assumed that the retirement experience of the various classes of property has been analyzed sufficiently to indicate what the mortality characteristics have been; that reasonable estimates have been made, both of total capitalization periods, corresponding with the average service lives resulting from these studies, and of future capitalization periods, corresponding with the indicated life expectancies; and that 6% has been adopted as a reasonable rate of return.

It is also assumed that: (1) A complete engineering study has been made of each of the groups of property comprising the existing plant; (2) estimates have been made of the increase or decrease in cost at the current price level, as compared with the estimated cost of reproduction of the existing facilities, that would result from the utilization of the most economical equipment of each type available as a result of progress in the arts and of capacity no greater than sufficient to meet the requirements of the service; and (3) corresponding estimates have been made in each case of the savings in operating expenses that could be accomplished if this most economical equipment were utilized.

For example, it is assumed that it has been estimated that the same service as is rendered by the existing coke ovens and related facilities could be performed by a modern installation costing $2,500,000 as compared with an estimated cost of reproduction of $2,900,000 for the existing installation, and that at the same time a saving in operating expense of $45,000 per yr could be accomplished. Similarly, it is assumed that it has been determined that the service rendered by the existing cast iron mains, 12 in. or less in diameter,and by all existing wrought iron mains could be furnished more economically by the use of steel mains, and that single mains of adequate size could be substituted for parallel mains in some locations, with reduction in large main capacity in certain areas where the population and consumption have declined —all at a cost for distribution mains of, approximately, $14,200,000 compared with an estimated reproduction cost new of the existing mains of $18,600,000, but without any substantial difference in operating expenses.

It is also assumed that: (*a*) Among other modernizations, installation costs would be increased by $235,000 and operating expenses decreased by $58,000 if modern water gas sets equipped with reversed air blast and waste heat boilers, and, to the extent that it would be economical, with mechanical grates, were substituted for existing installations of older types without these improvements; (*b*) installation costs would be decreased by $110,000 and operating expenses decreased by $24,000 through the substitution of liquid purification for dry box purification; (*c*) the use of a modern positive displacement type station meter in place of several old type wet meters would reduce installation cost by $62,000, and operating expense by $500; and (*d*) the installation of modern electric motor driven and gas engine driven booster units in place of certain old steam engine driven units, with gas fired boilers for holder heating, and related changes at holder stations, would reduce installation costs by $600,000 and operating expenses by $16,000.

It is also assumed that similar engineering determinations have been made for every other group of items of property considered, after investigation, to have been affected by obsolescence (progress in the arts) or changes in demand; and that future capitalization periods have been adjusted to take into account any known requirement for retirement of property in advance of dates corresponding with mortality experience because of requirements of public authority or desirability of improving the quality or reliability of service. In utilizing these modern types of equipment as a basis for estimating the cost of providing the existing service economically, the attempt has been made to use only types of equipment that had records of proved use and that were generally accepted

as representing the current standard of design practice in modern plants. It is not believed that effect should be given in such estimates to recent scientific discoveries that have not passed beyond the laboratory or the pilot plant stage, or to initial installations of equipment for the operation of which no substantial amount of experience is yet available.

For the purpose of concentrating attention upon the subject of this paper, the writer has intentionally refrained from discussing the reasonableness of any of the assumptions referred to previously, or any of the numerous interesting collateral subjects that suggest themselves, such as the price level applied in estimates of cost, intangible values (including going concern value), and limitations applicable to enterprises subject to industry obsolescence where competition of alternative services or alternative objects of consumer expenditure would prevent the earning of a return on even the most economical facilites.

The resultant estimates of reproduction cost new of the existing facilities, the estimated cost new of the facilities required to render the service most economically, the reasonable total capitalization periods, the amount of the annuity on the cost of the facilities required to render the service most economically, the estimated annual savings in operating expenses allocated to the various classes of property involved, the amount available for capitalization to measure the value of the service and the service value of the existing facilities, the future capitalization period of the existing facilities, the corresponding capitalization factor at 6% return, the estimated service value of the existing facilities, the amount of accrued depreciation in the existing facilities, and the percentage of depreciation are shown in Table 1, in accordance with the New York State Public Service Commission classification of accounts, adopted for purposes of illustration. In Table 1, attention is invited particularly to Cols. 8 and 9. Where obsolescence or change in demand can be measured by comparison with the fixed charges and operating expenses of equipment of the most economical type available as a result of progress of the arts, and of capacity adapted to the requirements of the service—which is of such a nature that it would be classified in a single account identical with that in which the existing property is classified—no problem of classification arises and service value can be determined by the direct application of the suggested formula. Where, however, the most economical facilities of appropriate capacity are of such a nature that they would be classified in a number of different accounts and where the reduction in the total cost of fixed charges and operating expenses is attributable to the entire group of property, the suggested formula for service value is directly applicable only to the group as a whole, and no strictly logical basis exists for distributing the resultant combined annuity and savings in operating expenses among the individual accounts in which the most economical property is classified. It is desirable, however, that such distribution be made for the purposes of both contributing toward the utilization of the results for accounting purposes, as referred to hereafter, and of giving an indication of the service value of the subdivisions of the property for any purpose.

It has been the writer's practice in studies made in this field to combine groups of property, which are affected as a group by obsolescence or change in

TABLE 1.—SUMMARY OF COMPUTATION OF SERVICE VALUE AND ACCRUED

Account		Cost of reproduction new of existing plant[a] (thousands of dollars)	Cost of most economical facilities to furnish service[a] (thousands of dollars)	Capitalization period for most economical facilities[b] (years)	Annuity factor at 6% for period in Col. 5	Annuity on cost of most economical facilities[c] (thousands of dollars)
No.	Designation					
(1)	(2)	(3)	(4)	(5)	(6)	(7)
311	Land	2,500	2,500	Indefinite	0.06	150
312	Structures and improvements	4,000	3,500	60	0.06187	216.5
313	Boiler plant equipment	900	1,000	35	0.06897	69
314	Other power equipment	300	400	50	0.06344	25.4
316	Coke ovens	2,000	1,800	35	0.06897	124.1
317	Producer gas equipment	400	400	38	0.060736	26.9
318	Water gas generating equipment	1,300	1,500	40	0.06646	99.7
321	Coal, coke, and ash handling equipment	2,200	2,200	55	0.06254	137.6
322	Gas reforming equipment	60	60	40	0.06646	4
323	Purification equipment	1,600	1,500	34	0.06960	104.4
324	Residual refining equipment	600	550	48	0.06390	35.1
325	Other production equipment	1,440	1,390	34	0.06960	96.7
	Total production plant	17,300	16,800			1,089.4
341	Land	500	500	Indefinite	0.06	30
342	Structures and improvements	4,500	4,200	70	0.06103	256.3
	Total storage plant	5,000	4,700			286.3
361	Land	300	300	Indefinite	0.06	18
362	Structures and improvements	900	800	60	0.06187	49.5
363	Mains	18,600	14,200	65	0.06139	871.7
364	Pumping and regulating equipment	1,000	800	35	0.06897	55.2
365	Services	6,800	6,200	48	0.06390	396.2
366.1	Meters	4,800	4,700	30	0.07265	341.5
366.2	House regulators	8	8	48	0.06390	0.5
366.3	Meter installations	1,792	1,792	50	0.06344	113.7
	Total distribution plant	34,200	28,800			1,846.3
370	Land	300	300	Indefinite	0.06	18
371	Structures and improvements	2,200	2,000	60	0.06187	123.7
372	Office furniture and equipment	500	500	20	0.08718	43.6
373	Transportation equipment	200	200	10	0.13587	27.2
374	Stores equipment	15	15	25	0.07823	1.2
375	Shop equipment	25	25	28	0.07459	1.9
376	Laboratory equipment	20	20	22	0.08305	1.7
377	Tools and work equipment	220	220	12	0.11928	26.2
379	Miscellaneous equipment	20	20	22	0.08305	1.7
	Total general plant	3,500	3,300			245.2
	Total plant in service	60,000	53,600			3,467.2

[a] Estimated. [b] Assumed same as for existing facilities except for account 363, for which total capitalization land excluded this value is 0.27218. [c] With land excluded this value is 0.11162. [d] With land excluded this

demand, in "study projects" for which total annuities, total savings in operating expenses, total service values, and total loss in service value have been computed. These total annuities and total savings in operating expenses have then been apportioned among the several accounts affected in proportion to the equal annual payment annuity on the estimated cost of reproduction new of the corresponding existing property. The results of such "study project" computations have then been combined with the results of similar computations for cases in which the most economical property of appropriate capacity fell in a single account identical with that in which the existing property was

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Redistribution of Col. 7 (thousands of dollars)	Annual operating expense savings by most economical facilities[c] (thousands of dollars)	Amount available for capitalization,[d] Col. 8 − Col. 9 (thousands of dollars)	Future capitalization period for existing facilities (years)	Present worth factor at 6% for period in Col. 11	Service value of existing facilities Col. 12 × Col. 10 (thousands of dollars)	Accrued Depreciation	
						Col. 3 − Col. 13 (thousands of dollars)	Col. 14 / Col. 3 (%)
(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
150	150	....	150	Indefinite	16.667	2,500	....	....
216.5	231.1	20	211.1	45	15.456	3,262.8	737.2	0.18430
69	64.8	20	44.8	21	11.764	527	373	0.41444
25.4	20.3	10	10.3	39	14.949	154	146	0.48667
124.1	121.2	30	91.2	25	12.783	1,165.8	834.2	0.41710
26.9	26.9	....	26.9	26	13.003	349.8	50.2	0.12550
99.7	99	50	49	29	13.591	666	634	0.48769
137.6	136.3	2	134.3	42	15.225	2,044.7	155.3	0.07059
4	4	....	4	29	13.591	54.4	5.6	0.09333
104.4	98.1	30	68.1	21	11.764	801.1	798.9	0.49931
35.1	35.8	3	32.8	39	14.949	490.3	109.7	0.18283
96.7	97.4	5	92.4	29	13.591	1,225.8	184.2	0.12792
1,089.4	1,084.9	170	914.9			13,271.7	4,028.3	0.23285[e]
30	30	....	30	Indefinite	16.667	500	....	....
256.3	257.5	3	254.5	49	15.708	3,997.7	502.3	0.11162
286.3	287.5	3	284.5			4,497.7	502.3	0.10046[f]
18	18	....	18	Indefinite	16.667	300	....	....
49.5	50.1	2	48.1	43	15.306	736.2	163.8	0.18200
871.7	871.7	....	871.7	57	16.065	14,003.9	4,596.1	0.24710
55.2	56.6	12	44.6	28	13.406	597.9	402.1	0.40210
396.2	396.2	....	396.2	33	14.230	5,637.9	1,162.1	0.17089
341.5	341.5	....	341.5	17	10.477	3,577.9	1,222.1	0.25460
0.5	0.5	....	0.5	39	14.949	7.5	0.5	0.06250
113.7	113.7	....	113.7	31	13.929	1,583.7	208.3	0.11624
1,846.3	1,848.3	14	1,834.3			26,445.0	7,755.0	0.22675[e]
18	18	....	18	Indefinite	16.667	300	....	....
123.7	125	3	122	35	14.498	1,768.8	431.2	0.19600
43.6	43.6	....	43.6	12	8.384	365.5	134.5	0.26900
27.2	27.2	....	27.2	6	4.917	133.7	66.3	0.33150
1.2	1.2	....	1.2	16	10.106	12.1	2.9	0.19333
1.9	1.9	....	1.9	14	9.295	17.7	7.3	0.29200
1.7	1.7	....	1.7	12	8.384	14.3	5.7	0.28500
26.2	26.2	....	26.2	6	4.917	128.8	91.2	0.41450
1.7	1.7	....	1.7	12	8.384	14.3	5.7	0.28500
245.2	246.5	3	243.5			2,755.2	744.8	0.21280[h]
3,467.2	3,467.2	190	3,277.2			46,969.6	13,080.4	0.21717[i]

period of existing facilities is 80 years. [c] Col. 6 times Col. 4. [d] As service value of existing facilities. [e] With value is 0.22876. [h] With land excluded this value is 0.23275. [i] With land excluded this value is 0.23104.

classified, and with those of corresponding computations on the portions of the property assumed, after study, not to be affected by obsolescence or change in demand. This procedure has been followed in the example presented herein and is the basis of the classifications in Cols. 8 and 9, Table 1. It is hoped that criticisms, comments, and suggestions regarding this convention and procedure will be included by those who discuss this paper.

In Table 2 is shown, for the same example, the application of the percentages of accrued depreciation arrived at in Table 1 to the assumed original cost of the property for the purpose of estimating the depreciation reserve requirement for

TABLE 2.—SUMMARY OF COMPUTATION OF DEPRECIATION RESERVE PURPOSES OF HYPOTHETICAL

Account		Original cost of existing plant (thousands of dollars)	Accrued depreciation
No.	Designation		
(1)	(2)	(3)	(4)
311	Land	2,500	....
312	Structures and improvements	6,000	18.430
313	Boiler plant equipment	900	41.444
314	Other power equipment	500	48.667
316	Coke ovens	2,300	41.710
317	Producer gas equipment	300	12.550
318	Water gas generating equipment	1,500	48.769
321	Coal, coke, and ash handling equipment	2,500	7.059
322	Gas reforming equipment	40	9.333
323	Purification equipment	1,600	49.931
324	Residual refining equipment	600	18.283
325	Other production equipment	1,460	12.792
	Total production plant	20,200	23.397
			26.701[d]
341	Land	500	....
342	Structures and improvements	3,400	11.162
	Total storage plant	3,900	9.731
			11.162[d]
361	Land	300	....
362	Structures and improvements	800	18.200
363	Mains	11,600	24.710
364	Pumping and regulating equipment	700	40.210
365	Services	4,600	17.089
366.1	Meters	3,800	25.460
366.2	House regulators	8	6.250
366.3	Meter installations	992	11.624
	Total distribution plant	22,800	22.644
			22.946[d]
370	Land	300	....
371	Structures and improvements	1,900	19.600
372	Office furniture and equipment	450	26.900
373	Transportation equipment	200	33.150
374	Stores equipment	15	19.333
375	Shop equipment	25	29.200
376	Laboratory equipment	20	28.500
377	Tools and work equipment	175	41.450
379	Miscellaneous equipment	15	28.500
	Total general plant	3,100	21.048
			23.304[d]
	Total plant in service	50,000	21.842
			23.537[d]

[a] Col. 4 times Col. 3. [b] Col. 7 times Col. 6.

accounting purposes. Also shown, based on assumptions as to the corresponding percentages of accrued depreciation at the end of the following year, and the net debit to retirements during the year, are the amount of the depreciation reserve requirement at the end of the following year and the corresponding annual depreciation expense during such year. Table 2 is included for the

REQUIREMENT AND ANNUAL DEPRECIATION EXPENSE FOR ACCOUNTING MANUFACTURED GAS COMPANY

Depreciation reserve requirement[a] (thousands of dollars) (5)	Original cost of existing plant one year later (thousands of dollars) (6)	Accrued depreciation one year later (%) (7)	Depreciation reserve requirement one year later[b] (thousands of dollars) (8)	Net debit to depreciation reserve (thousands of dollars) (9)	Annual depreciation expense[c] (thousands of dollars) (10)
....	2,500	....	....	....	....
1,105.8	6,100	19.2	1,171.2	5	70.4
373	920	41.8	384.6	2	13.6
243.3	510	48.6	247.9	2	6.6
959.3	2,350	42.2	991.7	2	34.4
37.6	310	12.6	39.1	1	2.5
731.5	1,520	49.3	749.4	1	18.9
176.5	2,540	7.6	193	10	26.5
3.7	40	10.7	4.3	....	0.6
798.9	1,620	50.6	819.7	5	25.8
109.7	610	18.5	112.8	7	10.1
186.8	1,490	13.7	204.1	30	47.3
4,726.1	20,510	24.0	4,917.8	65	256.7
		27.3[d]			
....	500	....	....	....	....
379.5	3,460	11.0	380.6	10	11.1
379.5	3,960	9.6	380.6	10	11.1
		11.0[d]			
....	300	....	....	....	....
145.6	810	18.1	146.6	3	4
2,866.4	12,000	23.9	2,868	150	151.6
281.5	720	39.8	286.6	7	12.1
786.1	4,700	16.5	775.5	200	189.4
967.5	3,850	26.8	1,031.8	70	134.3
0.5	8	7.5	0.6	....	0.1
115.3	1,012	11.3	114.4	10	9.1
5,162.9	23,400	22.3	5,223.5	440	500.6
		22.6[d]			
....	300	....	....	....	....
372.4	1,920	19.9	382.1	5	14.7
121.1	460	28.4	130.6	20	29.5
66.3	210	31.5	66.2	40	39.9
2.9	15	20.0	3	1	1.1
7.3	25	30.8	7.7	1	1.4
5.7	20	30.5	6.1	1	1.4
72.5	165	48.2	79.5	16	23
4.3	15	30.0	4.5	1	1
652.5	3,130	21.7	679.7	85	112.2
		24.0[d]			
10,921	51,000	22.0	11,201.6	600	880.6
		23.6[d]			

[a] Col. 8 − Col. 5 + Col. 9. [d] With land excluded.

purpose of illustrating the application of the suggested approach to the establishment of the depreciation reserve requirement and annual depreciation expense in case the existing classifications of accounts and the desire to have depreciation reserves represent "approximately the amount of depreciation actually existing in the property" are continued without change. However,

both the regulatory commissions and the public utility industry would be well advised to separate completely the accounting requirements from the methods of estimating service value. This separation would permit the adoption of some relatively simple accounting convention such as straight-line age-life depreciation for accounting purposes without any implication that the resultant depreciation reserves, original cost less such reserves, and depreciation expense charges had any effect on valuation or rate making.

Closing Comment

Attention is called to the fact that the result of the calculations shown in Table 1 can be stated either as a determination of the reasonable annuity for depreciation and return which, in combination with the operating expenses, other than depreciation, of the most economical property establish a reasonable level of gross revenues without reference to the valuation or operating expenses of the existing property; or as a determination of the service value of the existing property for use in the conventional formula in which reasonable rates are based upon a return on the fair value of the property. The result will be identical provided annual depreciation expense is treated in a corresponding manner in the two statements, and may be illustrated as follows:

A. Gross Revenue Based Upon Value of Service.—

Operating expenses of existing plant, exclusive of depreciation	$ 8,500,000
Assumed saving by most economical facilities	190,000
Operating expenses of most economical facilities	$ 8,310,000
Annuity on cost of most economical facilities	3,467,200
Total gross revenue	$11,777,200

B. Gross Revenue Based Upon Return on Fair Value.—

Operating expenses of existing facilities, exclusive of depreciation	$ 8,500,000
Annual depreciation and return on existing facilities	3,277,200
Total gross revenue	$11,777,200

This illustration, together with Tables 1 and 2, also calls attention to the effect on income statements that could result from the adoption of this suggestion as a result of the lack of correspondence between the actual incidence of retirements and the incidence of retirements corresponding with the theoretical depreciation expense included in the estimated reasonable gross revenue. Thus, in the example, where net debits to retirement reserve have purposely been assumed as substantially in excess of what would correspond with the theoretical depreciation expense, the operating income for the year would be reduced to $2,397,200, or 4.9% or the assumed fair value of $46,969,600, instead of the 6% assumed as a fair rate of return (still a little more than 6% of

the average original cost less depreciation reserve requirement). In other words, this procedure would have the disadvantage, if it is such, of introducing fluctuations into the operating net income (as retirement expense accounting did) as the actual retirements varied above or below those corresponding with the theoretical depreciation. It would also introduce further irregularities with the irregular occurrence of new developments in progress in the arts and changes in demand.

On the other hand, it would have advantages over other procedures of reflecting proportionate losses in value more nearly when they occur, and tending to maintain reserves more nearly at the same proportion of original cost as accrued depreciation was of reproduction cost new. Whatever may be the advantages or disadvantages of the adoption of this suggestion from the accounting and financial points of view, it is believed that it offers a solution of the valuation and rate problems which is worthy of further study and development.

The point of view expressed herein has been presented in the form of testimony in the United States District Court for the Eastern District of New York, in a case involving the valuation of certain gas and electric distribution property taken through condemnation by the federal government. In this final opinion of the District Court in this proceeding, after stating that:

> "* * * [the writer's] understanding of the basic purpose of rate regulation is to give the consuming public the benefit of competition based upon the value of the service if it were to be furnished according to operating expenses and fixed charges as resulting from the functioning of such facilities of the most modern and economical type."

the Court stated: "That would seem to provide a fair standard of value for present purposes" (65 Federal Supplement 333 and 71 Federal Supplement 248). The Attorney General of the United States has filed notice of appeal in this proceeding.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

REPORTS

REVIEW OF SLOPE PROTECTION METHODS

REPORT OF THE SUBCOMMITTEE ON SLOPE PROTECTION OF THE COMMITTEE ON EARTH DAMS OF THE SOIL MECHANICS AND FOUNDATIONS DIVISION

SYNOPSIS

A summary of available recorded information on the protection of the slopes of earth dams against the disrupting effects of waves, and the erosive effect of rain, wind, and frost, is presented in this report.

The types of protective covers investigated are:

1. Dumped stone riprap;
2. Hand-placed riprap;
3. Grouted riprap;
4. Concrete slabs and blocks;
5. Porous concrete paving;
6. Bituminous paving;
7. Grass seeding, sodding, and other planting; and
8. Miscellaneous covers.

Also included are a suggested procedure to enable the designer to compute wave heights, lengths, and velocities, and the application of certain formulas to the design of riprap slope protection.

Specific design data relating to the design of riprap and other types of slope protection are very meager. It is hoped that this paper will stimulate discussion leading to the discovery of additional data. The field of full-scale experimentation appears to offer the greatest promise in developing reliable and economical designs of slope protection; but, until facilities for such experimentation are available, the designer will need to exercise caution in the treatment of these problems.

GENERAL

The continuous action of moving water is very searching. The construction of an earth dam across a stream increases the depth and expanse of water and presents an upstream slope which is subject to wave attack and erosion.

NOTE.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **November 1, 1948.** Progress reports are published in *Proceedings* only.

An upstream slope protection must be designed to resist these forces and also the effects of reservoir drawdown. The effects of a decreasing fetch with the reservoir drawdown will affect the design of the slope protection on the lower levels of the dam slope. The downstream slope requires protection from wind and rain and the disturbances which may be caused by high tailwater.

Extent of the Slope Protection

Upstream slope protection should extend from the maximum height to which wave action can reach to a point a safe distance below minimum pool level. Slope protection usually terminates at a toe wall formed of large stones or of concrete and further protection is sometimes provided by an embankment berm at this level. The upper edge of the protection should not, as is sometimes done, be tapered in thickness to merge into the roadway. The top edge of the protection is likely to be the most vulnerable part as it will be subjected to maximum wave action; and, if any overtopping were to occur, it would start there.

In determining the required freeboard of the dam, the effect of seiches, wind setup, wave height, and wave ride up must be considered. It has been customary to ignore seiche effects except where experience has shown such rises to exist. This information is available for large bodies of water such as the Great Lakes, but any allowance for seiche effect in an artificial body of water would be arbitrary. Wind setup can be computed by the Zuider Zee formula—

$$S = \frac{V^2 F}{1{,}400\, D} \cos A \qquad (1)$$

in which S is the setup, in feet above pool level; V is the wind velocity, in miles per hour; F is the fetch, in miles; D is the average depth of water, in feet; and A is the angle of incidence of the waves. It has been customary to take D as two thirds of the maximum depth of the reservoir at the heel of the dam.

A method of computing wave heights is given in the succeeding paragraphs of this paper. The wave ride up is taken by the Corps of Engineers as 1.4 times the height of wave, measured vertically from the still water level. California practice is to use 1.5 times the wave height. The practice of the Corps of Engineers is to take maximum flood pool level, add the necessary allowances for wave setup, wave height, and wave ride up, and make this level the top of the earth embankment. This is usually a minimum of 5 ft above the maximum flood pool level.

The slope protection then extends from the top of the embankment to a point at least 5 ft below the minimum level of the pool. It is advisable to key the lower edge of slope protection into a berm or to provide a toe wall of concrete or of large stones to serve as a buttress against possible undermining and consequent raveling of the slope protection. The purpose of this toe wall or buttress is not, as sometimes stated, to support the weight of the riprap above, except possibly in the case of monolithic concrete slab slope protection. Unless each square yard of the protection has sufficient stability to support itself on the slope of the embankment, the protection cannot be considered sound.

Destructive Forces That Act on Embankment Slopes

The principal destructive force that acts on the water face of an earth dam is that due to wave action. Of secondary importance is the destructive action of erosion from rainfall, water currents, reservoir drawdown, frost, and wind. An adequate protection against wave action will usually protect the slope from the less destructive forces. When protection against waves is not required, such as on the downstream face of the dam, the problem is primarily one of wind and rainfall erosion, and a cover of vegetation, gravel, or crushed stone will usually be adequate.

Wave Action.—Wave force may be exerted and transmitted in all or a combination of several ways. The force may be:

(1) A static pressure due to the head of a column of water;
(2) The result of the kinetic impact of rapidly moving particles of water;
(3) The impact of debris floating on the surface of the water and hurled by the waves against the structure; or
(4) The reversal of head caused by the subsidence of the mass of water causing pressures to be built up by seepage trapped within the dam.

No destructive force surpasses wave action in so unerringly detecting each weak place or faulty detail of construction in the structure on which it acts.

The main sources of information on this subject are the observational data and empirical formulas based on the work of Thomas Stevenson.[1] The work of Thomas Stevenson was supplemented later by D. D. Gaillard[2] and the late D. A. Molitor,[3] M. ASCE. Their studies were directed toward the design of sea walls and breakwaters, but are, to some extent, applicable to the study of wave action on dam slopes.

The force exerted by waves on structures depends on the mass of the impinging water and its velocity. The size of a wave for any particular locality varies with the velocity of the wind, the duration of the storm, the depth of water, and the distance over which the wind can act. This distance is called the "fetch" and is generally defined as the normal distance from the windward shore to the structure being designed. The effective "fetch" may have a curved path, as in the case of the wind sweeping down a slightly curved river valley between high land ridges. The probability that the effective fetch may be determined by a curved path, rather than by a straight one, may explain some of the exceptionally high waves sometimes observed in long winding reservoirs.

Some reservoirs may have a widely fluctuating water level, with a corresponding fluctuation in the effective fetch. As the water level is lowered, it is entirely conceivable that a submerged body of land will be exposed and the fetch will be reduced. With a decrease in the fetch, the amount of slope protection required will be decreased materially and it will be possible to decrease the depth of the cover and the stone sizes for the lower levels, thus permitting the more effective use of a wide range of stone sizes.

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[1] "The Design and Construction of Harbours," by Thomas Stevenson, A. C. Black, Edinburgh, 1874.

[2] "Wave Action in Relation to Engineering Structures," by D. D. Gaillard, *Professional Paper No. 31*, Corps of Engrs., U. S. Army, U. S. Govt. Printing Office, Washington, D. C., 1904.

[3] "Wave Pressures on Sea-Walls and Breakwaters," by David A. Molitor, *Transactions*, ASCE, Vol. 100, 1935, p. 984.

For a given wind velocity, V, in miles per hour, and a fetch, D, in statute miles, the height of wave, measured from trough to crest, h, in feet, may be estimated from the following formulas which Mr. Molitor based on the formulas of Mr. Stevenson after introducing wind velocity as a variable and using statute miles instead of nautical miles: For values of D greater than 20 miles—

$$h = 0.17\sqrt{V\,D} \quad \text{(2a)}$$

and, for values of D less than 20 miles—

$$h = 0.17\sqrt{V\,D} + 2.5 - \sqrt[4]{D} \quad \text{(2b)}$$

A formula proposed by Mr. Molitor gives the following relation between the ratio of wave length and height to wind velocity:

$$\frac{L}{h} = \frac{840}{V} \quad \text{or} \quad L = 840\,\frac{h}{V} \quad \text{(3)}$$

in which L is the length of wave, in feet, measured from crest to crest; h is the height of wave, in feet, measured from trough to crest; and V is the wind velocity, in miles per hour. This formula is based on observations by Captain Gaillard in relatively shallow inland lakes.

There have been many formulas proposed for computing wave heights under varying wind and fetch conditions. The following are two fairly recent ones:

$$h = (0.0335\,V - 0.28)\sqrt{D} \quad \text{(4)}$$

$$h = \frac{D^{0.37}\,V^{0.48}}{C} \quad \text{(5)}$$

Fig. 1 shows a comparison of these formulas and the Molitor revision (Eqs. 2) of the Stevenson formula. The Wolf formula (Eq. 4) gives higher wave heights than the Molitor formulas. The formula (Eq. 5) proposed by W. P. Creager, M. ASCE (in a letter to the chairman of the subcommittee dated March 25, 1946), can be used for any degree of conservatism desired by simply changing the coefficient, C. With a value of C equal to 3.41, the Creager formula gives results that are almost identical, except for the shorter fetches, to those obtained from the Molitor formulas. For fetches shorter than 5 miles, the Molitor formula gives larger wave heights than does the Creager formula. The Molitor formula gives a 2.5-ft minimum wave height for the smallest fetch. Although this does not appear to be rational, it does have the effect of establishing a more conservative basis for designing slope protection for short fetches.

The Creager formula presents a promising form for a formula to be developed should future observations and developments warrant a radical change in the Molitor formula.

Although the Stevenson-Molitor formula is still in general use, experience has proved that its results do not assure reliable indications of critical wave heights, several occasions having been reported in which wave heights have greatly exceeded those given by the formula. Accordingly, wave heights computed from the Stevenson-Molitor formula should be considered as approxi-

mate, and more representative of average occurrences than the usual events involved in structural design. This is further supported by recent research conducted by the Scripps Institution of Oceanography at La Jolla, Calif., under the auspices of the United States Navy.[4] There is some question whether these latter studies may be applied to inland waterways, where a precise determination of fetch is difficult and where the effect of topography on wind and waves has not yet been fully explored. Further research and study of this problem are important to the engineering profession. Until such time as additional

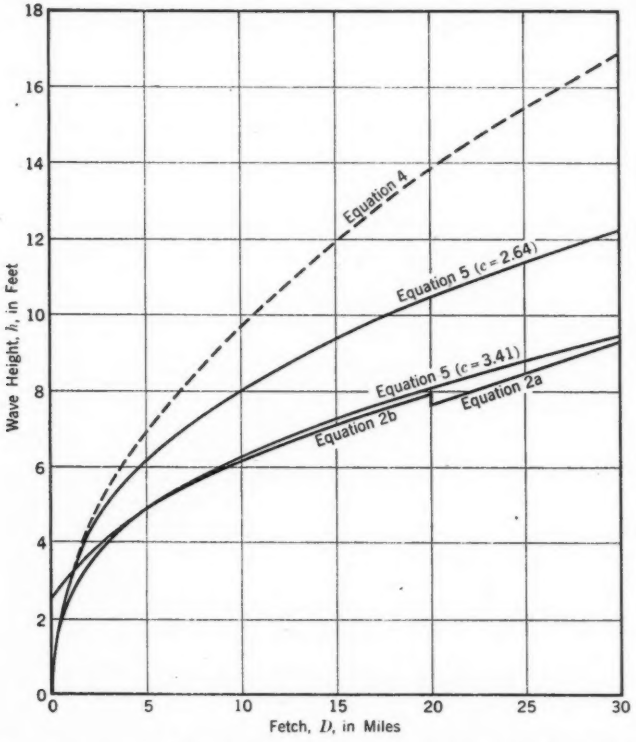

FIG. 1.—COMPARISON OF WAVE HEIGHT FORMULAS FOR $V = 100$ MILES PER HR

information is available, the use of the Stevenson-Molitor, the Scripps, or other empirical methods must be tempered with judgment.

Having obtained the wave dimensions, the next step is to evaluate the force that such a wave exerts when it strikes an obstruction. Mr. Molitor accepts the following conclusion arrived at by Captain Gaillard:

> "A mass of water projected with a certain velocity against a submerged plane surface of considerably smaller area than the cross section of the mass, can produce no greater pressure than would be caused by the steady flow at the same velocity of a current against a submerged plane surface of equal area and similar to the first."

[4] "Estimating Storm Conditions in San Francisco Bay," by J. A. Putnam, *Transactions*, Am. Geophysical Union, April, 1947.

It follows that the force a wave exerts on riprap stones on the face of a dam cannot be greater than that of a current flowing at a velocity equal to the velocity of the water particles of the wave.

Based on formulas derived by Mr. Molitor, with factors added to apply to conditions prevalent in fresh water reservoirs, engineers of the Tennessee Valley Authority have used the following formula to compute the equivalent current velocity, v, of a wave such as may be generated in a fresh water reservoir:

$$v = \left[3.25\, c + 11.35 \left(\frac{h}{L} u \right) \right] \sqrt[4]{h\, L} \quad \ldots\ldots\ldots\ldots\ldots\ldots (6)$$

in which v is the velocity, in feet per second.

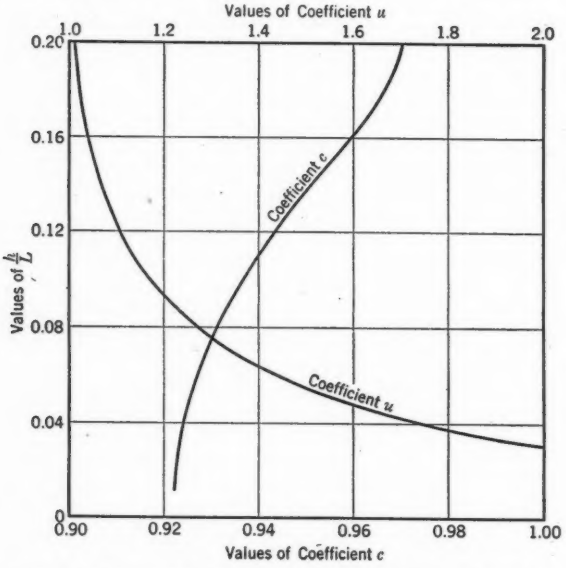

FIG. 2.—EQUIVALENT CURRENT VELOCITY OF WAVE VALUES OF COEFFICIENTS u AND c IN EQ. 6

Coefficients c and u vary with the ratio of depth of water and wave length. For average conditions prevailing in fresh water reservoirs adjacent to dams, this ratio may be expressed in terms of the wave height, h, and of the length, L, which can be computed from Eqs. 2 and 3. From recorded observational data, it is possible to express the coefficients c and u in terms of h and L. Fig. 2 expresses this relationship graphically.

Where slope protection is subjected to the combined action of waves and flowing water, the effective component of the current velocity should be added to the velocity computed from Eq. 6.

APPLICATION OF THE THEORY OF WAVE ACTION TO SLOPE PROTECTION DESIGN

The function of an adequate slope protection is to withstand and dissipate the energy of the moving mass of water resulting from wave action,

and to return the mass of water to its source without scouring or eroding the embankment.

By far the most popular types of slope protection used are dumped and hand-placed stone riprap. The design of riprap protection has been based largely on "rule of thumb" and past experience. The lack of specific information on the behavior of riprap when subjected to wave action has resulted in either the use of ultraconservative amounts of stone or the use of an inadequate amount or a doubtful type of riprap.

A paper by S. Ishbash[5] gives a report of experiments and model tests of the action of stones dumped in flowing water. One of the basic factors considered was

> * * * the ability of the individual stone, located on the downstream face of a rock fill, to resist displacement from the hydraulic forces due to overflow and to percolation of water through the body of the dam."

Recent model studies in France[6] tend to confirm the correctness of Mr. Ishbash's conclusions. These model studies were made to determine the design of dumped stone cofferdams for the Gneissiat Dam on the Rhône River and were verified by actual construction experience on the cofferdams.

The empirical formula developed by Mr. Ishbash is as follows:

$$v = Y Z \sqrt{d} \quad \text{or} \quad d = \frac{v^2}{(Y Z)^2} \ldots\ldots\ldots\ldots\ldots\ldots (7)$$

in which v is the velocity of flow, in feet per second; d is the diameter of stone (assumed as being of spherical shape); Y is a constant which varies for different dam slopes; and Z is a constant which varies for different specific gravities of stone used:

$$Z = \sqrt{2 g \frac{s - w}{w}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

in which g is the acceleration due to gravity; s is the weight of a cubic foot of stone; and w is the weight of a cubic foot of water.

Mr. Ishbash gives the following values of Y:

For slope 1 on 1 ..... $Y = 0.86$
For slope 1 on 12 ..... $Y = 1.20$

Engineers of the Tennessee Valley Authority have used Eq. 7 as a guide in selecting size of stone required to resist the water velocity derived from Eq. 6. Values of coefficient Y for slopes between 1 on 1 and 1 on 12, for lack of experimental data, are interpolated by assuming that Y varies inversely as the tangent of the slope.

The conditions of stability of the stones in the Ishbash experiments were different from those of typical riprap. The stones were dropped into flowing water, thus being exposed to the water velocity over their entire area before coming to rest on the submerged slope. Riprap stones are at rest and pre-

[5] "Construction of Dams by Dumping Stones Into Flowing Water," by S. Ishbash, Leningrad, 1932.
[6] *La Houille Blanche*, January–February and March–April, 1947

sumably are well nested among other stones on the slope. The cited test conditions, therefore, are somewhat more severe than those that would prevail on a typical dam slope. It is obvious that conditions during the incidence of an incoming wave are much different from those under which the Ishbash experiments were made and that the downrush of water as the wave subsides is more directly comparable in action to the flow of water through and over the submerged rock pile which Mr. Ishbash employed. It has even been suggested that the stability of an enrocked slope is more dependent on the steepness of that slope than on wave height. This theory would tend to eliminate wave height as a factor affecting stability and would scarcely be concurred in by anyone familiar with ocean breakwater design. The enforced use of single masses of concrete weighing up to 200 tons to top out some of the Pacific coast harbor jetties is convincing evidence that wave height is the most important factor in determining stability. It should be noted that some of the Pacific coast jetties have impervious cores and gentle slopes, making them roughly comparable in outline to earth dams. The Ishbash formulas are used, not because they are considered the final answer to the complex problem of slope protection design, but because they appear to offer the most direct approach to this problem.

If an experimental wave tank capable of producing long waves at least 6 ft high were available, it would be possible to determine the correct design for any type of slope protection and the economies possible of attainment for the large number of earth dams now in the planning stage would amount to

TABLE 1.—Relationship Between Fetch, Wave Height, Determined from

Fetch (miles), D	Wind (miles per hr), V	Wave height (ft), h	Wave length (ft), L	$\frac{h}{L}$	c	u	Equivalent current velocity, v
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	50	2.70	45.4	0.059	0.927	1.45	13.4
1	75	2.97	33.2	0.089	0.934	1.21	13.4
2.5	50	3.15	53.0	0.059	0.927	1.45	14.3
2.5	75	3.57	40.0	0.089	0.934	1.21	14.7
2.5	100	3.93	33.0	0.119	0.943	1.11	15.4
5.0	50	3.70	62.2	0.059	0.927	1.45	15.5
5.0	75	4.30	48.1	0.089	0.934	1.21	16.2
5.0	100	4.81	40.3	0.119	0.943	1.11	17.1
10.0	50	4.52	76.0	0.059	0.927	1.45	17.1
10.0	75	5.38	60.3	0.089	0.934	1.21	18.1
10.0	100	6.09	51.1	0.119	0.943	1.11	19.2
15.0	50	5.20	87.3	0.059	0.927	1.45	18.4
15.0	75	6.23	70.0	0.089	0.934	1.21	19.4
15.0	100	7.12	59.8	0.119	0.943	1.11	20.7
20.0	50	5.77	97.2	0.059	0.927	1.45	19.4
20.0	75	6.97	78.1	0.089	0.934	1.21	20.5
20.0	100	7.99	67.2	0.199	0.943	1.11	22.0

many millions of dollars. The 6-ft wave would be full scale at most artificial reservoirs and it would furnish a reliable indication of the effect of waves at least 10 ft high. There is, to the knowledge of the subcommittee, no reliable evidence of a true wave height exceeding 10 ft on any artificial reservoir.

Dumped Riprap Stone Slope Protection

The term "dumped stone riprap" is defined as stones dumped in place to protect a bank or fill from the action of waves and flowing water. The stone is placed on a properly graded filter blanket. Any hand labor involved consists of moving the stones to conform to the neat lines and to correct gross segregation of sizes in localized areas. The resistance of the riprap to displacement depends on four variable characteristics:

(1) Weight or size of the individual stones;
(2) Depth of the riprap;
(3) The slope of the embankment on which the riprap rests; and
(4) The stability and efficacy of the filter blanket on which the riprap is placed.

The first three characteristics may be expressed as functions of the displacing forces produced by waves and flowing water according to the methods previously outlined. The design of the filter blanket is discussed subsequently.

From the Ishbash theory it is possible to compute the theoretical weight and dimensions of stones that will resist displacement by wave action resulting

AND REQUIRED THICKNESS OF 1 ON 3 SLOPE PROTECTION AS THE ISHBASH THEORY

Theoretical Stone Sizes						Recommended depth of riprap (ft)
155-lb stone		165-lb stone		175-lb stone		
d (ft)	w (lb)	d (ft)	w (lb)	d (ft)	w (lb)	
(9)	(10)	(11)	(12)	(13)	(14)	(15)
1.52	288	1.38	227	1.26	185	1.5
1.52	288	1.38	227	1.26	185	1.5
1.72	418	1.57	333	1.43	272	2.0
1.84	512	1.66	395	1.51	318	2.0
2.02	677	1.82	521	1.66	423	2.0
2.04	698	1.85	547	1.68	440	2.0
2.22	900	2.02	712	1.83	565	2.0
2.48	1,250	2.25	982	2.04	790	2.0
2.48	1,250	2.25	982	2.04	790	2.5
2.81	1,820	2.55	1,430	2.31	1,070	3.0
3.13	2,520	2.84	1,980	2.58	1,590	3.0
2.87	1,940	2.61	1,540	2.36	1,220	3.0
3.20	2,690	2.90	2,110	2.62	1,670	3.0
3.66	4,020	3.30	3,090	3.00	2,500	3.5
3.20	2,690	2.90	2,116	2.56	1,550	3.0
3.57	3,730	3.24	2.930	2.94	2,350	3.5
4.11	5,180	3.73	4,480	3.39	3,600	4.0

from wind velocity and fetch conditions. It is recognized that the specifications for dumped riprap should prescribe a more or less uniform gradation to obtain a dense, well-graded mass and to permit the use of quarry run stone. The larger stone should be roughly uniformly distributed throughout the mass to stabilize it. Until additional experiments or experience can supply better information, it is suggested that the following rules be used in establishing the required gradation of riprap stone:

1. Stone equal to or larger than the theoretical size computed from Eq. 7 with a few larger stones, up to about twice the weight of the theoretical size, tolerated for reasons of economy in the utilization of the quarried rock, should make up 50% of the rock by weight.
2. The gradation of the lower 50% should be selected to satisfy the filter requirements between the riprap and the upper layer of the filter blanket.
3. The depth of the riprap should accommodate the theoretically sized stone with a tolerance in surface elevations designed to permit the occasional oversized stones mentioned in rule 1.
4. Within the preceding limitations the gradation from largest to smallest sizes should be quarry run.

Some designers may wish to use a variation of these rules in so far as the size of the maximum size stone is concerned. It is believed that a specification based on the foregoing principles will be sufficiently liberal to insure economy in utilizing practically all the stone obtainable from a quarry and at the same time will secure conservative slope protection.

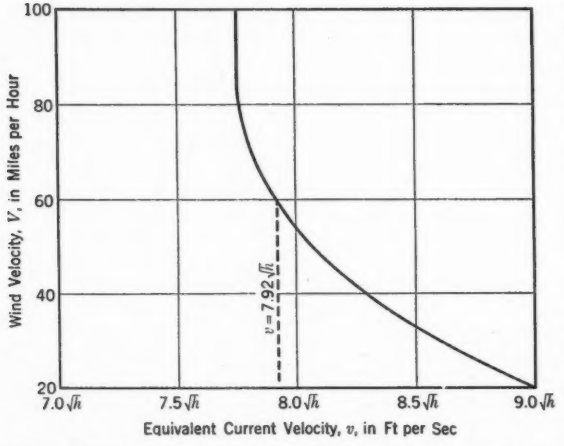

FIG. 3.—EQUIVALENT WAVE VELOCITY AS A FUNCTION OF WAVE HEIGHT FOR ANY GIVEN WIND VELOCITY

Table 1 has been prepared using Eqs. 2 to 7 to show the theoretical stone sizes necessary for the dumped riprap requirements for various wind velocity and fetch requirements. The stone sizes shown are the sizes computed from Eq. 7. The recommended depth of riprap is based on the use of the theoretically spherical stone of the theoretically stable weight.

In selecting the probable wind velocity to which a given installation of slope protection may be subjected, all conditions affecting exposure must be considered. It is believed that from a geographical standpoint no locality is safe from the occurrence of at least occasional winds of 100 miles per hr or greater velocity. However, the particular site may be so sheltered topographically

that the reaches on which the winds must play to produce waves can never be subjected to sustained winds of high velocity. Under these conditions the use of a V-value of 75 miles per hr or even 50 miles per hr may be justified.

It will be noted that the ratio h/L and the coefficients c and u remain constant for any given wind velocity regardless of the amount of fetch. By combining Eqs. 3 and 6, the equivalent wave velocity may be expressed as a function of the wave height for any given wind velocity. This relationship is shown graphically in Fig. 3.

Figs. 3 to 5 developed from the Ishbash theory may be used to find the dumped riprap requirements for various dam slopes subjected to wave action resulting from a wide range of wind and fetch conditions. It is known that the riprap on many earth dams is consistent with the results which will be derived from these curves. It is not claimed, however, that success might not be found through an entirely different approach to the problem.

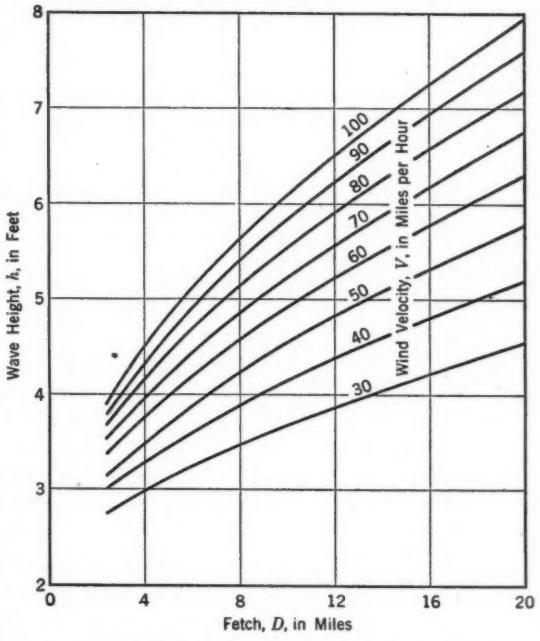

FIG. 4.—WAVE HEIGHTS FOR VARIOUS CONDITIONS OF WIND VELOCITY AND FETCH (EQ. 2b)

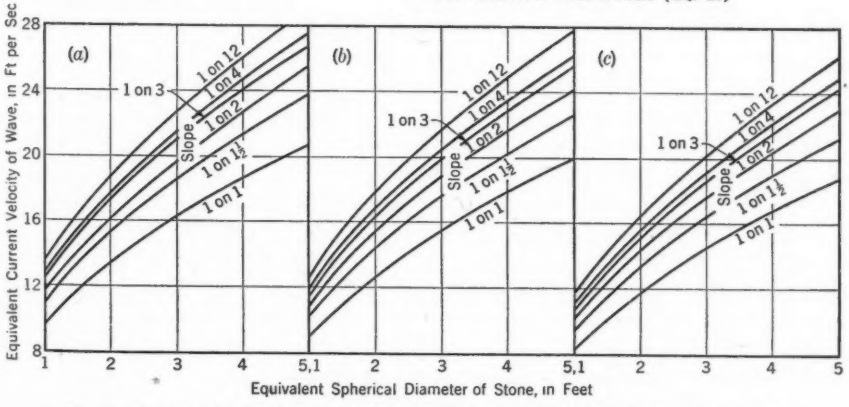

FIG. 5.—SIZE OF STONE THAT WILL RESIST DISPLACEMENT FOR VARIOUS VELOCITIES AND SLOPES FOR STONE DENSITY OF:
(a) 175 Lb per Cu Ft
(b) 165 Lb per Cu Ft
(c) 155 Lb per Cu Ft

If the problem is to design the slope protection for the channel downstream from the embankment, it will be necessary to obtain the wave height, h, and the wave length, L, from model tests under maximum discharge conditions from the spillway or outlet works, or both. The equivalent water velocity due to the wave is computed from Eq. 6, making the necessary corrections for the direction of the wave in relation to the bank and the velocity of flow along the bank. Eddy currents due to the discharge from the spillway or outlet works are likely to be far more important than wave and scour action in a typical discharge channel.

It is desirable to coordinate the design of riprap protection, as much as possible, without sacrificing safety, with the best and most economical construction practice in the quarrying, transporting, and placing of rock. An effort should also be made to use local bank-run gravel, quarry fines, or crusher-run rock in the filter blanket. The stone sizes tabulated in Table 1 for the stated conditions of wind and fetch are sizes that can be readily handled by the usual quarry loading equipment.

Stone for riprap should be hard and durable and should be able to resist long exposure to weathering. Most of the igneous and metamorphic rocks and many of the limestones and sandstones make excellent riprap. Shale is not a suitable stone. Limestones and sandstones that have shale seams are generally undesirable. It is advisable to study the use of local rock in existing structures as a guide in determining the suitability of the rock as riprap. The slope protection survey being conducted by the Corps of Engineers has found several instances of long satisfactory service by quarry rocks which when first exposed would not pass any standard "soundness" test. The rock had "case-hardened" with age and was in better condition after many years of exposure than when first quarried.

Construction specifications for riprap and filter blankets should be written very carefully to avoid costly delays and perhaps unsatisfactory execution of the work required to give an adequate and economical protection.

The foregoing design analysis is based on assumptions and empirical formulas developed by Messrs. Ishbash and Molitor. The results of such designs are believed to be conservative. Recorded observational data on the behavior of riprap installations on earth embankments are scanty and not sufficiently detailed to form the basis of an exact design procedure. Until a more successful material or a more suitable method of application is developed, it appears that dumped riprap on a filter blanket will continue to be the most desirable slope protection material.

The added expense incurred by hauling suitable riprap stone from distant sources may be justified where job conditions indicate that the use of other methods of slope protection are of doubtful value. This is especially true when the structure to be protected is subject to excessive wave action or where failure would be disastrous. The failure of the concrete block protection on Kingsley Dam[7] in Nebraska is a classic illustration of the hazards involved.

[7] "Protecting Upstream Slope of Kingsley Dam," by Henry H. Jewell, *Civil Engineering*, November, 1945, p. 493.

Demonstrated inability to make effective repairs to the articulated concrete mat finally resulted in the use of dumped stone riprap.

Tests are now being planned by the Corps of Engineers to check theories on riprap design. At the present time, a wave tank which can produce waves of a maximum height of 1.7 ft is available, and an apparatus which can produce 6-ft waves is contemplated.

Hand-Placed Riprap Slope Protection

Hand-placed riprap consists of stones laid carefully by hand in a more or less definite pattern with a minimum amount of voids and with the top surface relatively even. As in the case of dumped riprap, the stones should be placed on a filter blanket of properly graded gravel or crushed stone of adequate depth. There should be enough voids or openings in the surface of the riprap to vent the subsurface properly. The size of the largest openings will govern the maximum size of the filter layer. The depth of the riprap or size of stone required to resist displacement by wave action is generally specified as one half of that required for dumped riprap. A minimum thickness of 12 in. is recommended.

The best hand-placed riprap approaches good dry rubble in quality and appearance. The cost of hand-placed riprap per cubic yard is considerably greater than the cost of dumped riprap, lower material cost being offset by the increased handling and labor costs—particularly when the rock is large and heavy equipment is needed to place it. When the nearest source of suitable stone is located far from the site, it may be economical to use hand-placed riprap despite the higher unit cost for labor and material. Use of hand-placed riprap may be economical also when a one-man size stone is required and stone of the proper size and shape is readily available.

It should be recognized that hand-placed riprap is not as flexible as dumped riprap, since it cannot readjust itself as well as dumped riprap in the event of foundation displacement or local failure. In some old installations of hand-placed riprap recently inspected, sufficient damage was found to justify the conclusion that hand-placed material is no more effective than an equal depth of dumped material.

Grouted Riprap

Grouted riprap is hand-placed or dumped riprap with the joints filled with a Portland cement mortar. The grout is broomed and rodded into the voids. As in the case of dumped and hand-placed riprap, it is placed on a gravel or crushed stone filter blanket. Some of the holes or joints are left ungrouted so that the subsurface can be vented to avoid uplift from hydrostatic pressure.

The use of grouted riprap may be justifiable where large enough stones are not available to satisfy the requirements for dumped riprap, or when the stone available is not adaptable to hand placing. The quality of the stone and mortar used should satisfy the requirements of good concrete materials. Grouted riprap is especially suited for the paving of high-velocity channels. It is rarely used on dams or high embankments except for repairs.

Concrete Paving

Concrete paving deserves serious consideration for slope protection where the use of riprap is too expensive because of high transportation costs.

The success of concrete pavement as a slope protection medium depends on the evaluation of the field conditions and the assumptions made as to the behavior of the dam fill and the ability of the concrete to resist cracking and deterioration.

For the purpose of discussion, the design of concrete pavements will be divided into two distinct types—the slab or jointed type, and the monolithic or unjointed type. Both types are so constructed and so placed on the embankment that they are normally not subjected to displacement by wave force. The factors to be considered in the design of either type are as follows:

(1) The pressure due to the wave mass and impact;

(2) The reversal of head caused by the rapid subsidence of the wave or by rapid reservoir drawdown causing pressures to be built up by seepage trapped under the slab;

(3) Stresses caused by the unequal settlement of the underlying material; and

(4) The severity of the climate and the relative durability of the concrete.

Concrete Slab Pavement.—In this type of pavement the slabs are designed as independent units to resist all the forces to which they may be subjected. Because of their ability to settle with the blanket, any superimposed water load can be transmitted directly to the underlying foundations. The slabs in some cases may have to be provided with vents to prevent the building up of excessive heads resulting from wave seepage or rapid drawdown of the reservoir. The slabs must be heavy enough to resist the wave forces, or flotation resulting from any residual uplift pressures that the vents cannot successfully prevent. Unless the slabs are massive, reinforcement will be necessary to prevent cracking. In this connection it is interesting to note that local observers at several dams with slope protection of this type have stated that waves striking the slopes at an acute angle were far more effective in "blowing up" slabs than were those striking normal to the face. This was especially true at Belle Fourche Dam, in South Dakota, where the waves were reported as reaching 8 ft in height. It is easy to note that a wave traveling along the slope might simultaneously place a maximum ride up and a minimum trough closely adjacent, and so subject an area of slab to maximum hydrostatic uplift. The concrete should be durable and well cured to insure resistance to weathering. A properly graded filter blanket is essential to prevent erosion of the embankment fill and to prevent the washing away of the filter material through the joints between the slabs. It is believed that jointed concrete slope protection is hazardous. It certainly will be costly if properly designed.

Monolithic Concrete Pavement.—In this type of pavement the concrete is laid as a continuous reinforced slab over the entire surface. Where construction joints are necessary, the reinforcing steel is made continuous through the joint and the joint is so designed that the tension value of the reinforcing passing

through the joint will exceed the tension value of the concrete. This latter precaution is to insure that contraction will be distributed through a large number of small concrete cracks rather than through a few large ones. The absence of joints or large cracks in the surface will minimize the entrance of wave seepage, consequently no vents or filter blanket should normally be required. A moderate amount of reinforcement is provided to resist temperature and shrinkage cracks. This reinforcement is depended on to prevent disintegration of the mat after unequal bank settlement has caused undulations in the surface of the mat. The reinforcement can be designed to resist uplift due to wave-induced hydrostatic differentials, as suggested in a letter to the subcommittee from L. F. Harza, M. ASCE, bearing in mind that only a limited area of slab can be under such stress at a given instant and that this stressed area borrows stability from the adjacent areas. The possibility of angled wave incidence must be considered. Observations of 3-in. and 4-in. concrete paving exposed to almost continuous wave action, including the traveling waves produced by passing vessels, indicate that sound concrete paving on a relatively impervious base is not subject to these hydrostatic uplift forces. Where the slab is breached by damage or open joints, there can be little doubt that the uplift will occur. Only one layer of reinforcement, consisting of about 0.5% steel in each of two bands, one parallel and one normal to the slope, is placed at mid-depth of the slab. Seepage from surface infiltration from the top of the embankment is assumed to drain out through the downstream face of the dam and the fluctuations in the reservoir level are assumed to be gradual enough so that seepage from the reservoir will escape through the same channels by which it entered without building up a head on the bottom side of the slab. The success of this type of pavement is dependent to a great extent on the ability of the pavement to maintain its monolithic form. There are several existing reservoir embankments on which monolithic concrete paving slabs about 8 in. thick have proved successful. Of these examples two, McKay Dam in Oregon and Lake Babcock in Nebraska, are in regions subject to severe heat and cold. None of these projects, to the knowledge of the subcommittee, is subject to rapid fluctuation of water-surface level such as might occur in some flood-control reservoirs. It might be remarked that a combination of yielding foundations, heavy wave action, and rapid flood-routing cycles would furnish the severest test for any slope protection, and that no type of slope protection could be considered for general use unless it has been proved sound and stable under exposure.

Whenever provision must be made for rapid water-surface fluctuations, it is believed that a drained filter bed should be provided under even the monolithic type of concrete pavement. The filter bed should conform to good practice for similar filter beds under riprap. Drainage would be effected by intercepting perforated drain tile placed at the base of the concrete facing and draining into the reservoir.

Either type of concrete pavement should be considered as a substitute for stone riprap when stone riprap would be too costly. Consideration should be given in the comparison to the probable life of the concrete slab as compared

with sound stone riprap. Any slope protection problem deserves special study with the objective of adequately protecting the embankment at minimum cost. The survey of the Corps of Engineers has revealed quite as many instances of extravagant as of deficient enrockment. In some cases 3 ft of costly rock was used on slopes that could have been safely protected with an 8-in. monolithic concrete slab at a fraction of the cost. In every case the probable useful life of the project, as governed by silting, replacement, etc., and the feasilibity of repair, as controlled by usage, should all be considered in choosing between rock and concrete. It would seem reasonable to predict a low maintenance life of at least 30 years for sound concrete protection and this could be greatly extended in some cases.

Concrete Blocks

Concrete blocks in the forms of hexahedrons or tetrahedrons that are precast and then either dumped or placed in a symmetrical pattern on a suitable filter blanket are sometimes used as a protective cover. As in the case of concrete slabs, the use of concrete blocks should be limited to sites where suitable riprap is not readily available. The hexahedral blocks may have square or rectangular cross sections.

Rectangular concrete blocks of a special type were used to protect the upstream face of Kingsley Dam because rock for riprap was not readily available. The blocks were provided with horizontal openings through which long wrought iron rods were passed in such a pattern that the riprap mat was effectively interlocked and yet remained flexible. They were laid on a bed of fine gravel, highly deficient in the coarser brackets. A short time after the reservoir was partially filled, waves from 8 ft to 9 ft high caused rather severe damage to the slope. An investigation of the damage resulted in the opinion that the fine gravel filter blanket material was washed out by the wave action, causing the concrete blocks to become disarranged and break up. A more detailed account of the design, failure, and repairs to this slope is presented elsewhere.[7]

The Corps of Engineers has used concrete tetrahedrons in practice and in experiments for the control of bank failures due to current erosion. Because of the shape of the blocks, they nest and wedge themselves tightly when dumped on a bank. The dumped tetrahedrons have some merit when used as an emergency measure to protect underwater banks where stone is not available. Their cost is too high to permit general use. When placed on a silty riverbank without a graded filter, they disappear with magical rapidity.

Porous Concrete Paving

Porous concrete is a concrete consisting of gravel with enough cement paste added to coat the particles of gravel. No sand is used and the gravel is graded to permit the maximum amount of porosity consistent with the desired strength. When properly made, it is relatively strong and nearly as pervious as ordinary gravel. A filter blanket of finer gravel and sand is generally required to protect the earth fill. On the recession of a wave, the water can readily drain out through the voids of the porous concrete, thereby preventing the formation of an excessive amount of hydrostatic head against the bottom of the slab.

Porous concrete is not as susceptible as solid concrete to cracks caused by temperature and shrinkage stresses. Because of the greater exposed area, any deleterious chemical contamination or calcium deficiency in the water will subject the porous concrete to rapid deterioration. The use of porous concrete should be confined to mild climates where the formation of ice is very rare and where the water is neither heavily contaminated with deleterious chemicals nor deficient in calcium.

Porous concrete was used on the upstream slope of the earth dam at the Santee-Cooper Project in South Carolina. The concrete was 8 in. deep and it was placed on a 4-in.-thick pea gravel filter blanket. The aggregate for the concrete was graded from $\frac{3}{4}$ in. to $\frac{1}{4}$ in. with 50% coarser than $\frac{3}{8}$ in. No expansion or contraction joints were provided. Tests have shown that this porous concrete loses strength very rapidly when exposed to water action, probably due to the great surface area exposed to leaching and chemical action. This work was performed during 1940 and considerable areas are now being replaced with dumped stone.

Bituminous Paving

Bituminous pavements for slopes have been built by applying premixed bitumens and aggregate or have been built in place by the penetration method. An insufficient amount of work has been done with bituminous pavement to advance it definitely beyond the experimental stage. Practically all the work of this nature done to date was concerned with revetment work to prevent scour by flowing water. It is doubtful whether a bituminous pavement can be built to withstand severe wave action. The problem of drainage is not serious for the penetration type, but the premixed type will present a problem of drainage similar to the monolithic concrete paving. The Corps of Engineers has used both premixed and penetration types of bituminous paving on levees and riverbanks where the wave action and current velocities were relatively small.

The standard Los Angeles (Calif.) Water Supply specification for slope protection slabs calls for the use of slow-curing liquid asphalt mixed in proportions varying from 4% to 6% with aggregate graded from the No. 200 sieve to the $\frac{3}{4}$-in. sieve with from 32% to 38% passing the No. 10 sieve. The liquid asphalt content is varied with the aggregate by the engineer in charge to produce a pervious blanket. The mixture is applied at between 300° F and 400° F. It is dumped on a carefully prepared subgrade, bladed to uniform thickness and, when the slope exceeds 20%, rolled first with a hot roller weighing 400 lb per lin ft and then with a cold roller weighing 1,200 lb per lin ft. The maximum thickness rolled is 3 in., with heavier mats built up by successive 3-in. applications. On testing the slab for porosity, it is required that water poured on it shall enter the slab and not run down the slope.

Filter Blankets

To prevent waves from washing out the underlying material through the voids in the riprap and destroying its support, all riprap or other porous protective cover should be laid on a bed of gravel or crushed stone. The material

should be graded in such a way as to prevent the infiltration of the dam material through the blanket, or the infiltration of the blanket material through the voids or openings in the slope protection.

Design of Filter Blankets.—The problem of designing a filter blanket under a protective cover for the surface of an earth dam is somewhat similar to the design of filter underdrains for the downstream toe of an earth dam. The required gradation of layers to produce a stable filter has been the subject of many investigations. Investigations, including those of Karl Terzaghi,[8] Hon. M. ASCE, G. E. Bertram,[9] and the United States Waterways Experiment Station,[10] at Vicksburg, Miss., have established definite rules for the selection of grading curves for the layers of material used in various types of filters. The findings of these investigations were tested by the Corps of Engineers at the Beach Erosion Board Laboratory, in Washington, D.C., to determine their application to the selection of filter blanket materials for use under a riprap protective cover on an earth dam. At first the tests consisted of subjecting a prototype section of a riprapped dam face to a flow of water over the surface. Later the section of dam face was subjected to wave action in a wave tank. In these tests the gradation of the layers in the filter blanket was varied until a stable condition was achieved.

All the investigators followed the criteria previously established by Professor Terzaghi of expressing the results of these tests in the form of a filter ratio. The filter ratio is defined as the ratio of the 15% particle size of the coarse layer to the 85% particle size of the finer layer. (The 15% particle size is a size which is larger than the finer 15% of the material and the 85% particle size is the size which is larger than the finer 85% of the material.)

In the case of a filter blanket for the protection of the face of an earth dam the tests confirmed that a filter ratio of 5 or less between the successive layers will result in a stable condition. This filter ratio requirement applies between the dam fill material and the filter blanket, between successive layers of filter blanket materials if more than one layer is used, and between the filter blanket and the protective cover. In addition to meeting the aforementioned size specifications, the grain size curves for the various layers should be roughly parallel to minimize the infiltration of the finer material into the coarser materials.

The number of layers of filter blanket material needed to satisfy the filter requirements will depend on the gradation of dam material and the openings or voids in the protective cover. More than one layer of filter may be desirable in some cases because of limitations in commercially available materials and because of the danger of segregation of materials having unusually wide gradations.

The thickness of the filter blanket will vary according to the grading of the material and the care with which it is placed. If the blanket is placed in one layer a thickness of from 9 in. to 15 in. should suffice. If more than one layer

[8] *Transactions*, ASCE, Vol. 100, 1935, p. 1391.

[9] "An Experimental Investigation of Protective Filters," by G. E. Bertram, *Soil Mechanics Series No. 7*, Graduate School of Eng., Harvard Univ., Cambridge, Mass., January, 1940.

[10] "Investigation of Filter Requirements for Underdrains," *Technical Memorandum No. 183-1*, U. S. Waterways Experiment Station, Vicksburg, Miss., November 1, 1941.

of material is required to satisfy filter requirements the individual layers should range from 4 in. to 8 in. thick, depending on the size of the material and on the placing conditions.

Fig. 6 shows the grading curve for a two-layer filter blanket placed on a loess dam fill. A filter blanket consisting of one layer for this case will result in material with a wide size range and segregation of the material is apt to occur during the placing.

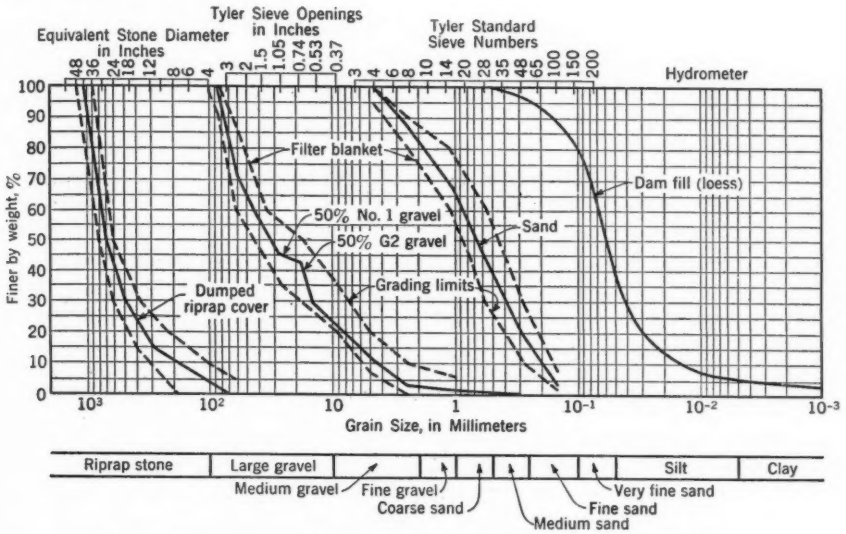

FIG. 6.—GRADATION OF TWO-LAYER FILTER BLANKET FOR LOESS DAM FILL

The foregoing discussion of filter blanket design is a summary of present information on the subject. It is hoped that large-scale prototype model tests in a wave tank will reveal additional information concerning desirable filter ratio requirements between the gravel blanket and the riprap layer.

PLANTING ON SLOPES

Planting on slopes includes:

(1) Seeding grasses or other crops;
(2) Sodding;
(3) Sprigging—Bermuda grass; and
(4) Planting with vines.

Planting as slope protection is resorted to for the protection of slopes against relatively mild erosive action resulting from the weathering action of rains, sun, or wind. It may also be used to protect flat slopes from mild and infrequent wave action. The downstream slopes of earth dams are generally planted above the maximum expected tailwater elevation. If the slope is exceedingly long, it may be desirable to place berms at regular intervals to prevent the accumulation of too much rain runoff over the lower part of the fill. Pipe drains may be necessary to drain the berms.

The following factors should be considered before a planting program is initiated:

1. Length of growing season;
2. Climate, temperature, total rainfall, and distribution of rainfall;
3. Type of soil available for planting; and
4. The slope of the surface and the ability of the dam material to store water for plant growth during dry spells.

Where climatic or soil conditions are not conducive to plant growth and where an abundant supply of large gravel is readily available, the use of a gravel or light riprap cover may prove to be both an economical and a desirable protection for downstream slopes.

Seeding.—Planting of slopes with grass and leguminous seeds is perhaps the least expensive and the most popular form of planting. Too often, however, it consists merely of scattering the seeds and lightly raking them into the soil so that the chances of survival have been very slim. Like any good crop, the seeds should be planted in a soil that contains the proper plant food elements and that has the ability to absorb and hold water to sustain the plants. It is generally necessary to spread a layer of properly fertilized topsoil at least 6 in. thick over the surface of the dam. If the dam fill is such that it can be enriched with fertilizers and other soil amendments, the application of topsoil may not be necessary.

The type of seeds planted will depend on the location and climate. It is generally wise to investigate the surrounding regions to determine which grass or cover crop thrives in that particular locality.

Sodding.—Where an abundance of well-established sod is available, and where a good plant cover is required in the shortest time possible, the use of sod may be justified even though the first cost is greater. The sod is generally laid on a prepared subsoil that should contain at least 3 in. of good fertilized topsoil, and is applied in squares or in strips handled in rolls. The surface should be rolled to insure contact with the subsoil, and the sod should then be watered.

Sprigging with Bermuda Grass.—Sprigging consists of planting "sprigs" or clumps of grass about 4 in. square at 12-in. to 24-in. intervals. Bermuda grass is generally planted in this manner because of the difficulty of securing a good stand by seeding. This type of grass will spread along the surface and take root where it comes in contact with the soil, the strong wiry roots and matted surface growth affording excellent protection against erosion from surface drainage. Bermuda grass thrives in warm and relatively dry weather after it has established itself, but it is a good plan to plant a quick growing grass, such as winter rye, to give some protection while the Bermuda grass is establishing itself.

Planting with Vines.—The planting of slopes to vines for erosion control is tending to supplant grass plantings, especially for the steeper slopes. The vines most generally used are honeysuckle, matrimony vine, kudzu, Virginia creeper, and trumpet vine. All these vines are perennials and spread themselves by taking root as they grow along the ground. In from 2 years to 3 years the ground will be covered with a dense mat of vegetation.

Honeysuckle and matrimony vine retain their foliage throughout the year in the temperate climates and for this reason are to be preferred where good appearance is desired. To insure the best results, the vines should be planted in pockets of fertilized topsoil and kept moist until well established. For maximum cover in the shortest time, the vines should be planted at 2-ft to 3-ft intervals. It is well to protect the intervening spaces by planting a fast-growing grass seed, such as winter rye, and mulching the surface. In from 2 years to 3 years, the vines will crowd out the grass and any weeds which may have gained a foothold. Very little maintenance is required for slopes planted to honeysuckle or matrimony vines. Maintenance will consist of cutting or mowing the vines at the edges where they encroach on roads and structures. Honeysuckle has been used on several recent Tennessee Valley Authority earth dams with great success.

In recent years kudzu has been much publicized as a soil builder and erosion deterrent. It does best in warmer climates because its foliage dies with the first frost. Because of its rapid growing and spreading properties, it should be used with caution when the slopes adjoin roads or structures. Kudzu will send out vines from 75 ft to 100 ft long in a single growing season under ideal growing conditions and the vines will take root where they come in contact with the soil. It needs a well-prepared and fertilized soil during its starting period.

Miscellaneous Slope Protection

Because the protection of the dam slopes is such an important part of earth dam design, it is only natural that new schemes should be proposed, especially where riprap is unobtainable or costly.

Where suitable quarry stone is not available and where a plentiful supply of clay is available, the use of ceramic blocks of irregular shape manufactured at the site presents a promising source for experimentation. D. A. Buzzell,[11] M. ASCE, presented an interesting account of experiments that were performed prior to including ceramic blocks as riprap materials as one of several alternates on the bids for Kingsley (formerly Keystone) Dam in Nebraska.

The experiments consisted of fusing ordinary raw commercial bricks together. At the point of incipient fusion the brick united at points of contact or ran together to form a semisolid mass resembling blast furnace slag. The mass was then broken into irregularly shaped blocks to suit the weight requirements for dumped riprap. The finished product had a specific gravity somewhat less than stone. This handicap, however, was more than overcome by the following points of superiority:

1. The angular blocks had a greater tendency than stone riprap to interlock and they offered greater resistance to the passage of the fine material from the filter blanket.
2. The material was practically inert to chemical action and almost completely resistant to many repetitions of the freezing cycle or to sodium sulfate tests.

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[11] "Fused Ceramic Material for Riprap on Keystone Dam," by D. A. Buzzell, *Engineering News-Record*, June 2, 1938, p. 787.

3. The probable cost, except when excellent quarry stone is located close to the site, was apparently favorable to use of the ceramic material.

The deficiency in weight may be compensated for by increasing the thickness of the riprap.

Actual test results on representative tests of ceramic riprap are as follows:

Description	American Society for Testing Materials designation	Quantity
Absorption	C67-31	1.5%–12%
Soundness	T-35	15 cycles (negligible loss)
Wear	D2-33	6%
Toughness index	D-3-18	11

and the crushing strength was from 10,000 lb per sq in. to 25,000 lb per sq in.

It has been stated that very large masses of ceramic material cannot be burned because of the shrinkage characteristics of the clays. However, subcommittee members witnessed the preparation of several hundred tons of this material and sound blocks weighing up to 3 tons were quite easily obtainable.

Steel facings[12] have been used on a few gravel and rock fill dams to prevent or to control percolation through the dam. Such a facing should necessarily be capable of resisting wave action. The steel plates are anchored to a concrete cutoff wall at the toe and to a concrete curb at the crest. At the top, the plate may be curved to deflect wave wash away from the top of the dam. A steel facing is superior to a monolithic concrete face in that it can adjust itself to deformations resulting from fill settlement and temperature changes without the formation of cracks. The ability of the steel facing to withstand the direct wave pressure is susceptible to structural analysis.

Acknowledgments

Members of the subcommittee furnished much of the information contained in this paper. Material contributed by W. B. Allen, M. ASCE, was especially useful, and criticisms by Mr. Creager, T. A. Middlebrooks, Assoc. M. ASCE, Mr. Harza, and Mr. Bertram were freely used in the final draft. The assistance of A. C. Marane, Assoc. M. ASCE, in preparing this report is gratefully acknowledged.

Respectfully submitted,

W. B. Allen
George E. Bertram
R. F. Blanks
R. A. Sutherland
W. J. Turnbull
Dow A. Buzzell, *Chairman*

Subcommittee on Slope Protection of the Committee on Earth Dams of the Soil Mechanics and Foundations Division

[12] "Steel Facing for Dams," by R. T. Logeman, *Civil Engineering*, January, 1939, p. 7.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

FRICTION COEFFICIENTS IN A LARGE TUNNEL

Discussion

BY G. H. HICKOX, A. J. PETERKA, AND R. A. ELDER

G. H. HICKOX,[45] M. ASCE, AND A. J. PETERKA[46] AND R. A. ELDER,[47] ASSOC. MEMBERS, ASCE.—The interest shown in this paper, as evidenced by the questions asked, the criticisms made, and the new data presented, is gratifying to the authors.

Mr. Barrows raises an important point when he emphasizes the authors' use of "equivalent circular sections." This method of computation was chosen because it provided a simple relationship between cross-sectional area and hydraulic radius. Although the actual hydraulic radius was a trifle smaller than that obtained by assuming an equivalent circular area, the amount of the difference is small and depends on the variability of the overbreak. The irregularities of overbreak can scarcely be known in advance, but some estimate of the average diameter is always available. Lacking other knowledge, the designer will assume an ideal section for his calculations and will thus utilize the data in the same manner as for the calculations of the paper. It follows that the actual areas, including overbreak, should be used instead of the nominal area. Failure to observe this precaution may result in considerable error. Thus, the designer must estimate the probable overbreak for the particular rock he is working in, and for this particular type of excavation, and must add this overbreak to the nominal diameter.

Messrs. Bradley and Wing are to be commended for the data of Fig. 19 and the accompanying descriptions of tunnel surfaces. This chart, in a much more complete form, was available to the authors, by courtesy of the Bureau of Reclamation, before the Apalachia tunnel tests were made. It is unfortunate that limitations of space prevented showing the experimental points in Fig. 19 and it should be noted that many of the curves presented are supported

NOTE.—This paper by G. H. Hickox, A. J. Peterka, and R. A. Elder was published in April, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: June, 1947, by Weston Gavett; September, 1947, by W. R. Barrows, Hunter Rouse, Karl R. Kennison, E. J. K. Chapman, Julian Hinds, and William P. Creager and Stephen H. Haybrook; October, 1947, by G. S. Tapley; and November, 1947, by J. N. Bradley and S. P. Wing.

[45] Associate Director, Eng. Experiment Station, Univ. of Tennessee, Knoxville, Tenn.

[46] Hydr. Engr., Hydr. Laboratory, Bureau of Reclamation, Denver, Colo.

[47] Associate Hydr. Engr., TVA Hydr. Laboratory, Norris, Tenn.

by scattered data. It was in part the uncertainty caused by these scattered results and by the conflicting nature of the curves that prompted the tests reported in this paper.

The authors are indebted to Mr. Hinds for his clear understanding and elucidation of the practical problems surrounding the use of the $(f - \mathbf{R})$-equation for fluid resistance. The nature of the transition region still seems to be very imperfectly understood and much additional work remains to be done, both in laboratories and in the field, before the differences between the Nikuradse and the Colebrook types of transition are fully understood. It seems significant that, although the Apalachia tests were carefully made, they do not conform to either the Nikuradse or the Colebrook type of transition. In the meantime, designers should be aware of the possibilities implied by the data, as suggested by Messrs. Bradley and Wing, and should design with caution.

The tests on the unlined rock tunnel seem to indicate that in spite of the apparent good results the Nikuradse equation for f in terms of k/r for the completely turbulent region may break down for large values of k/r by the introduction of other effects as indicated by Professor Rouse. His suggestion that perhaps the irregularities of the tunnel wall constituted constrictions of the flow area is certainly worth consideration. The unlined rock tunnel was a succession of enlargements and constrictions caused by the method of driving it. The tunnel was at approximately its nominal diameter at each heading, and enlarged gradually to the next heading, with a very irregular surface as described in the paper and shown in Fig. 4. There seems to be little relationship between the size of the projecting rocks and the thickness δ' of the turbulent boundary layer. Perhaps the agreement with the Nikuradse equation is only fortuitous.

Mr. Chapman's suggestion that spiral flow may account for the high value of f in the concrete-lined tunnel is interesting. The cost of preparing for, and conducting, tests to investigate this point would be high because of the size of the conduit, the pressures in it, and its relative inaccessibility. No comparable data are available for the Ocoee No. 3 tunnel.

Mr. Gavett remarked on the successive approximations required by the piezometer locations. The location of the piezometers was a compromise between a complete set of pressure measurements such as might have been made in the laboratory and the practicalities of the field situation. Extensive installations are expensive and it was necessary to set up as few as would yield usable results. Observations were also a matter of serious consideration. The locations were isolated and access was over roads distinctly mountainous in character, some of which had been closed to public use and were no longer being maintained. The distance by such roads was several miles between points less than 7,000 ft apart along the tunnel. The distribution of personnel and equipment to these sites and the reading of gages at 1-min intervals for a continuous 8-hr period in rainy midwinter weather presented difficulties not encountered in the laboratory. Under these conditions it seemed much more reasonable to make a number of approximations than to obtain complete data.

With regard to Mr. Gavett's question about the exponent x in the equation $H_f = C\,V^x$, most of the available data indicate that x is 2 when the Reynolds number is large enough so that complete turbulence exists. For the part of the roughness curve lying on the smooth pipe curve, the exponent is less than 2. In the transition from "smooth" to "rough" flow, it may be either less than or greater than 2, depending on conditions that do not seem to be well understood. J. Nikuradse found a section of the transition from smooth pipe flow to complete turbulence where x was greater than 2. C. F. Colebrook found, however, that for commercial pipe x was everywhere less than 2 in the transition zone. In this field more data are needed, for unfortunately most flow occurs in the transition region, where knowledge of friction factors is least certain. This point is well brought out by Mr. Hinds in his excellent discussion.

The variation of Manning's n with velocity, mentioned by Messrs. Chapman and Hinds, is inherent in the equation whenever the frictional resistance does not vary with the square of the velocity. As a matter of fact, variation with the square of the velocity seldom occurs except for fully developed turbulence, which is beyond the range of most practical applications. Figs. 14 and 19 show the type of variation that may be expected. The surprising feature is not that Manning's equation does not fit these data, but that its inadequacy had not been discovered long ago.

It is hoped that, by the presentation of the data, use of the ($f - \mathbf{R}$)-relationship for tunnel friction calculations may be stimulated. The empirical equations of Manning, Scobey, Kutter, and Hazen-Williams are useful within the region of experiment, but there is considerable doubt that the coefficients are characteristic of the surfaces. These equations (except Kutter's) are exponentials and plot as straight lines on logarithmic paper. Consequently, the particular value of the exponent of S (or V, depending on the form of the equation) can be correct only for a relatively small range of velocities as indicated by the curves of Figs. 14 and 19.

The authors do not quite agree with Professor Rouse in his interpretation of the test results. His position appears to be that the Apalachia tunnel data should conform to the curves of Fig. 13. Curve (*d*) of this figure was obtained from Nikuradse's data on uniformly sanded surfaces, and curve (*b*), from Colebrook's data on certain commercial surfaces. Mr. Colebrook and C. M. White showed that the transition curve depended on the character of the surface. The authors see no reason why the data presented by them should conform to any of the transition curves previously published. Of far greater significance is the fact that, in all cases, the data for each type of surface tend unmistakably to approach the line representing a value of $\frac{1}{\sqrt{f}} - 2 \log \frac{r}{k} = 1.74$. The scattering of the data for the asphalt-lined tunnel at low Reynolds numbers is accounted for by the fact that the measured head losses were very small. The squares (Fig. 13) represent a maximum head loss of 0.123 ft; and the first four diamonds, a maximum head loss of 0.144 ft. The losses for the concrete-lined tunnel varied from 0.88 ft to 12.58 ft; and for the unlined rock, from 3.13 ft to 24.78 ft. These data scatter much less. The only inconsistency in them

is that they lie outside the range limited by the curves of Fig. 13. This is not unexpected because the surfaces tested probably differed from those represented in Fig. 13.

Since the data for all surfaces approach a value of 1.74 for $\frac{1}{\sqrt{f}} - 2 \log \frac{r}{k}$ as the Reynolds number increases, it seems clearly logical to estimate the apparent constant value of f in Fig. 10 for use in calculating k by Eq. 6. The results thus obtained are those of Table 9. There seems to be no more significance in the centroid of "Areas" A, B, and C of Fig. 13, as used by Professor Rouse, than in a centroid of Nikuradse's test data. Further, the similarity in the values of k for the asphalted surface, determined by Mr. Colebrook and the authors as 0.005 in. and 0.0035 in., respectively, cannot be ignored. Professor Rouse's value of $k = 0.00063$ in. needs explanation.

The authors believe that the test data have significance but not necessarily that described by the Colebrook transition function. These data have been presented in the hope that study of the transition region will be stimulated and that an explanation will eventually be evolved which will include the apparent deviations of the present contribution.

Professor Rouse's remarks concerning portrayal of Manning's n as a function of the Reynolds number appear irrelevant. At no point in the paper was n shown in that relationship except in the tables, where it might equally have been considered to be a function of gate opening or head loss. In spite of the fact that leading hydraulicians are turning toward the representation of surface roughness and frictional resistance in terms of f and the Reynolds number, there are still a great many designers who prefer to use empirical relationships such as Scobey's and Manning's equations. It was for this reason that Manning's n was shown as a function of velocity in Figs. 11 and 12. These plots show clearly that Manning's n is not a constant function of the surface and that is the reason they were included. It is true that the velocity for each tunnel is proportional to the Reynolds number. However, those engineers who still use Manning's equation, and for whose benefit Figs. 11 and 12 were included, are not likely to interpret the data in terms of the Reynolds number in spite of the proportionality that exists.

Mr. Tapley's discussion of bend losses is interesting and points out the extremely unsatisfactory state of knowledge in this field. In the tests made by the authors, the only independent determination of bend loss was at bend 1. Losses at the other bends were calculated in Table 5 by application of the data given by Mr. Creager and Joel D. Justin,[5] M. ASCE, in their Figs. 67 and 68. This procedure was followed to provide as good an estimate as possible of the loss chargeable to friction. The determination of bend loss is one of the weak points in the paper, but fortunately the bend losses were relatively small and errors in their determination were consequently unimportant. In addition to the discussion by Mr. Tapley, it might be pointed out that tests by Albert Hofmann[41] also indicated that the bend loss was materially affected by

[5] "Hydro-electric Handbook," by William P. Creager and Joel D. Justin, John Wiley & Sons, Inc., New York, N. Y., 1927, pp. 119–120.

[41] "Der Verlust in 90°—Rohrkrümmern mit gleichbleibendem Kreisquerschnitt," by Albert Hofmann, *Mitteilungen*, Hydraulischen Instituts der Technischen Hochschule, München, Heft 3, 1929.

the surface roughness of the bend, the bends with the rougher surfaces, surprisingly enough, having the lower bend-loss coefficients. This serves further to emphasize the need for definitive experimentation in this field.

The data presented by Mr. Kennison have been converted to terms of f and the Reynolds number as listed in Table 15. In making these calculations

TABLE 15.—CONVERSION OF DATA FROM TABLE 12

Line[a] No.	Q (cu ft per sec)	V (ft per sec)	s ($\times 10^3$)	f	Temperature (degrees C)	R ($\times 10^{-6}$)
1.....	869.9	6.81	0.580	0.0103	17	7.43
2.....	925	7.25	0.619	0.0096	17	7.91
3.....	916	7.18	0.596	0.0095	16	7.63
4.....	951	7.46	0.677	0.0100	15	7.72
5.....	956	7.49	0.673	0.0099	17	8.17
6.....	996	7.81	0.736	0.0099	20.5	9.30
7.....	996	7.81	0.733	0.0099	1.0	5.33

[a] Corresponds to Col. 1, Table 12.

the cross section was assumed to be an equivalent circle with the given area of 127.6 sq ft. The values of f are unusually close to the smooth pipe curve and it would be of interest to know the condition of this surface. The points are plotted in Fig. 20.

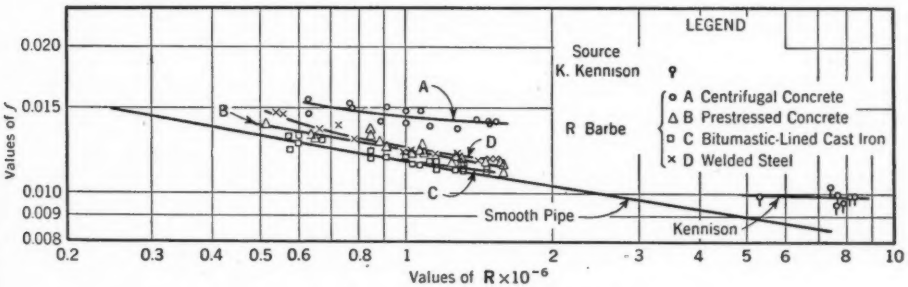

FIG. 20.—FRICTION FACTOR VERSUS THE REYNOLDS NUMBER FOR DATA OF K. KENNISON, M. ASCE, AND R. BARBE

Additional data on surface roughness of commercial pipes have recently been published,[48] and, in the belief that they have not come to the attention of the profession generally, are presented here for convenience of reference. The pipes were all 800 mm (31.5 in.) in nominal diameter and the test length was 200 m (656 ft). The results are shown in Fig. 20, together with the curve for smooth pipe flow. The surfaces are as follows:

A. Centrifugal concrete, of good average quality (new); 4-m (13.06-ft) lengths, with male and female ends, joined with aquarium cement, afterward smoothed on the inside, making a perfectly smooth joint.

B. Pre-stressed concrete (Freyssinet system); 6-m (19.85-ft) lengths with an inside surface formed during manufacture by very smooth helicoidal bands

[48] "La Mesure dans un Laboratoire des pertes de charge de conduits industrielles," by R. Barbe, *La Houille Blanche*, Mai-Juin, 1947, pp. 191–203.

48 mm wide with a pitch of 15 m. The bands were very smooth but were separated by fairly rough grooves about 5 mm wide and 1 mm deep. The joints were made by placing the sections end to end and sealing them with flexible rubber rings.

C. Cast iron with smooth bitumastic coating applied by a centrifugal process (new); 5-m (16.4-ft) lengths, very smooth joints, bell and spigot, with poured lead seal, smoothed inside.

D. Welded steel (new); 6-m (19.85-ft) lengths, each length composed of three sheets welded end to end and rolled. There were one longitudinal and two transverse welds in relief on the interior or each section. The joints were the same as for surface *B*.

It will be noted that the bitumastic-lined cast iron is the smoothest, and agrees very closely with the smooth pipe curve.

The authors wish to extend their sincere thanks to all who took the trouble to read and comment on a paper which, after all, merely added to the many conflicting data already existing without offering any explanation.

Corrections for *Transactions:* In October, 1947, *Proceedings*, on page 1307, Fig. 16, change "Values of $\frac{r}{l}\left(=\frac{1}{\Delta}\right)$, in Radians^{-1}" to "Values of $\frac{r}{L}\left(=\frac{1}{\Delta}\right)$, in Radians^{-1}"; on page 1308, Fig. 17, change "$\frac{r}{l} = 4.52$" and "$\frac{r}{l} = 1.67$" to "$\frac{r}{L} = 4.52$" and "$\frac{r}{L} = 1.67$"; in Table 13 and footnote 41, change "*Mitteilungen des Hydraulischen,*" to "*Mitteilungen,* Hydraulischen"; and, on page 1309, in the line above Eq. 12, change "represented" to "represented by" and, in Fig. 18, change "Values of $\frac{l}{r}(=\Delta)$, in Radians" to "Values of $\frac{L}{r}(=\Delta)$, in Radians" and change "$\frac{1}{8.85}$ (Fig. 2)" to "$\frac{1}{8.85}$ (Fig. 17)." In November, 1947, *Proceedings*, on page 1453, after the first sentence under "Curve 29" insert "(*Transactions*, ASCE, Vol. 109, 1944, p. 59)."

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DISCUSSIONS

STABILITY OF THIN CYLINDRICAL SHELLS IN TORSION

Discussion

BY R. G. STURM

R. G. STURM,[33] M. ASCE.—For the comments presented by the various discussers of this paper, the author wishes to express his sincere appreciation. Particularly does he wish to thank those who have presented contrasting views, because only in the light of such contrasts can a true value of any theoretical contribution to engineering science be determined. In connection with the discussion by Messrs. Batdorf and Stein, the author would like to mention that the significant difference between his work and that of Professor Donnell is that one single expression is used to cover the entire gamut of cylinder lengths from the very short to the very long, thereby eliminating any break or discontinuity at some arbitrary point where a cylinder is considered short or very long.

The discussion by Professor Murphy presents another very interesting point of view in that by the use of the algebraic equivalent to the curves some rather simple approximations are evolved. Such approximations are extremely valuable for the preparation of design charts.

Professor Donnell has presented a basic thought in connection with the value of discussions of technical papers—the significance of various points of view. Every solution requires certain assumptions and, in most cases, approximations which are usually different for different types of analysis. The author believes that Professor Donnell has made a real contribution to the general problem by pointing out a number of comparisons, and especially by his discussion of the effects of imperfections in actual cylinders.

The discussion by Mr. Moore is a welcome contribution to the general problem of the stability of thin cylindrical shells in that it gives for the first time a comparison of data obtained on complicated shells with the values computed by analytical methods. Although the discrepancies between measured and computed buckling loads are considerable, it is of interest to note that in all cases

NOTE.—This paper by R. G. Sturm was published in April, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1947, by S. B. Batdorf and Manuel Stein; November, 1947, by Glenn Murphy; December, 1947, by L. H. Donnell; and February, 1948, by R. L. Moore.

[33] Prof., Eng. Mechanics, and Research Prof. of Materials, Purdue Univ., Lafayette, Ind.

the actual buckling load is between 70% and 80% of the computed value, whereas the observed wave angle ranged from 1.26 times to 1.45 times that computed. Such consistency suggests more than a random relationship and indicates that perhaps some factor or factors are always present in the tests or should be included in the theory. The writer agrees with Mr. Moore in his statement about the effectiveness of longitudinal stiffeners and hopes that more experimental data will be forthcoming to serve as a basis for either establishing or modifying the theory to make it reliable for design.

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DISCUSSIONS

CONSOLIDATION OF FINE-GRAINED SOILS BY DRAIN WELLS

Discussion

BY REGINALD A. BARRON

REGINALD A. BARRON,[27] ASSOC. M. ASCE.—The benefits of drain wells have been brought out by Mr. Lane's discussion. Although, probably, they are not quantitatively correct, Eqs. 89, 90, and 91 are of great use qualitatively. The writer believes that Mr. Lane is correct in his statement that column action by the well backfill would redistribute the soil stresses and would result in a slower rate of consolidation. However, Fig. 8 indicates that the difference in consolidation rates is very minor and of no practical importance. In fact, such column action may be much less than the possible compressive effect of the horizontal seepage gradient. This possibility was not considered by the writer but its effect would be to compress the soil radially in a horizontal plane adjacent to the well where the seepage gradients are high. Because of this radial compression, the soil farther out will probably cause vertical settlement as it deforms radially in extension.

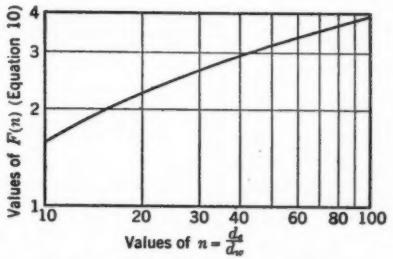

FIG. 14.—CURVE FOR THE DETERMINATION OF $F(n)$

Mr. Keene has discussed the use of consolidation drain wells on a highway project. The use of drain wells to accelerate the consolidation of foundations for highway fills is valuable, especially considering the effect of foundation consolidation on the finished quality of the pavement. On the other hand, such measures are not always necessary for earth dams, and it may be more economical to obtain stability by using flatter embankment slopes.

NOTE.—This paper by Reginald A. Barron was published in June, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1948, by Kenneth S. Lane; February, 1948, by Philip Keene; and May, 1948, by Walter Kjellman.

[27] Head, Special Studies Section, Embankment and Foundation Branch, Soils Div., U. S. Waterways Experiment Station, Vicksburg, Miss.

Mr. Kjellman discusses a very interesting development of the consolidation drain well method. It is possible that, should wick material and installation machines become available in the United States, sand wells may be outmoded. Mr. Kjellman states that "* * * great reliance is placed on the horizontal draining effectuated by coarse-grained layers" and "* * * the fine-grained soils in Sweden seem on the whole to be considerably less pervious than those in America." The writer is not in a position to state exactly what degree of reliance is placed on horizontal drainage in the United States. Furthermore, considering the variable geological history of this country, it is very possible that some of the clay deposits in the United States are quite similar to those of

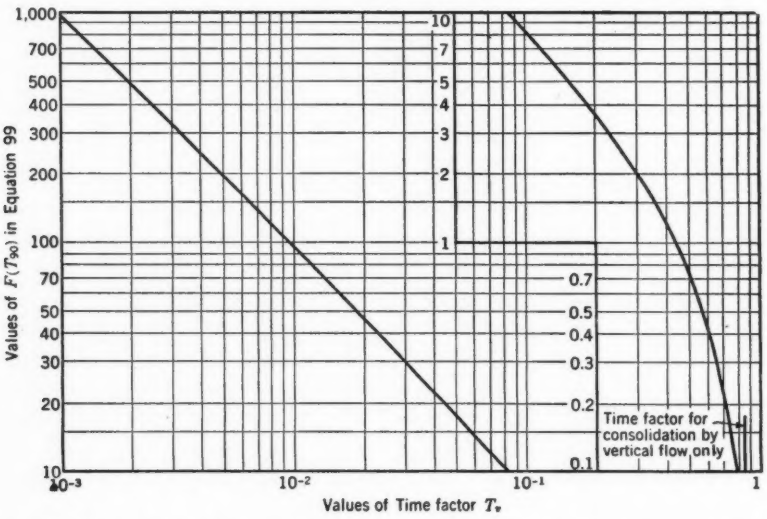

FIG. 15.—CURVE FOR THE DETERMINATION OF TIME FACTOR OF 90% CONSOLIDATION DUE TO COMBINED RADIAL AND VERTICAL FLOW

Sweden. The writer considered the effect of horizontal permeability in the theoretical developments because such conditions existed at sites for which the studies were made. For other cases this difference in permeability may not be so marked.

Mr. Lane requests additional curves indicating the effect of variables. Fig. 14 shows the variation of $F(n)$ as defined in Eq. 10 versus n which is used in the following development for instantaneously applied loads using Eq. 21 in which $\bar{u}_r$ is obtained from Eq. 8. For 90% average consolidation the average hydrostatic excess pressure is 10%. Thus:

$$\bar{u}_{r,z} = \bar{u}_z \epsilon^{\lambda} = 0.1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (93)$$

Expressions for the time factors are

$$T_h = \frac{k_h (1 + e) t}{\gamma_w a_v d_e^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (94)$$

and

$$T_v = \frac{k_v\,(1+e)\,t}{\gamma_o\,a_v\,H^2} \quad \ldots\ldots (95)$$

and, therefore,

$$T_h = T_v\,\frac{H^2}{d_e^2}\,\frac{k_h}{k_v} \quad \ldots\ldots (96)$$

and

$$\lambda = \frac{-8\,T_h}{F(n)} = \frac{-8\,T_v\,H^2\,k_n}{k_v\,d_e^2\,F(n)} \quad \ldots\ldots (97)$$

Consequently,

$$2.3026\,(1+\log_{10}\bar{u}_z) = \frac{8\,T_v\,H^2\,k_n}{k_v\,d_e^2\,F(n)} \quad \ldots\ldots (98)$$

or

$$\frac{(1+\log_{10}\bar{u}_z)}{T_v} = \frac{8\,k_h\,H^2}{2.3026\,k_v\,d_e^2\,F(n)} = F(T_{90}) \quad \ldots\ldots (99)$$

A curve of $F(T_{90})$ versus T_v is given in Fig. 15.

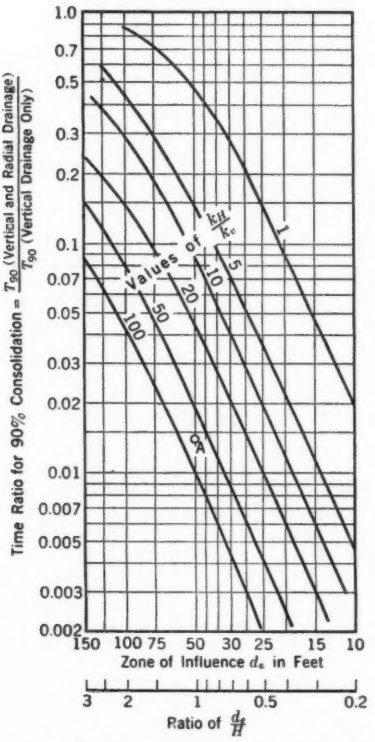

FIG. 16.—EFFECT OF WELL SPACING AND PERMEABILITY RATIO ON TIME REQUIRED FOR 90% CONSOLIDATION

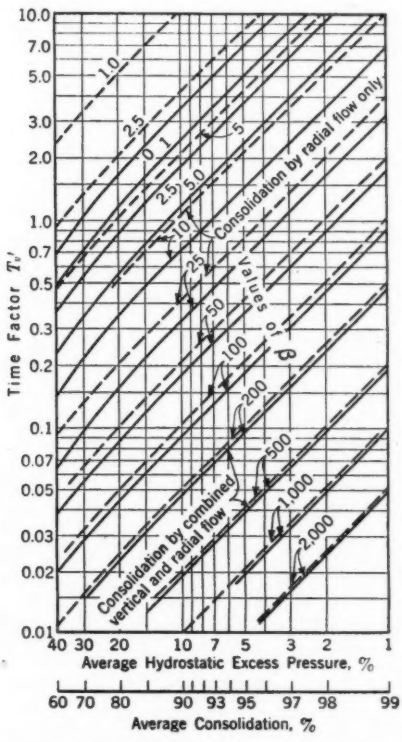

FIG. 17.—AVERAGE PERCENTAGE OF HYDROSTATIC EXCESS PRESSURE AND CONSOLIDATION AT END OF UNIFORM RATE OF LOADING PERIOD

Using the curve in Fig. 15, the curves in Fig. 16 for various permeability ratios were determined for the conditions $d_w = 1$ ft and $H = 50$ ft, with no smear or well resistance. The influence of permeability ratio k_h/k_v and well spacing is evident.

Point A in Fig. 16 was determined for the case of no smear but with well resistance for the condition that $k_w/k_v = 2{,}000{,}000$ and $k_h/k_v = 100$. Well resistance thus requires about 40% more time to reach 90% consolidation; for small ratios of k_h/k_v the effect would be less.

The curves in Fig. 17 for several values of

$$\beta = \frac{8\, k_h\, H^2}{k_v\, d_e^2\, F(n)} \qquad (100)$$

indicate the effects of k_h, k_v, H, d_e, and n on the average percentage of consolidation and average hydrostatic excess pore water pressure at the end of uniform rate of load application.

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DISCUSSIONS

TESTS OF TIMBER STRUCTURES FROM GOLDEN GATE INTERNATIONAL EXPOSITION

Discussion

BY COMMITTEE OF THE SAN FRANCISCO (CALIF.) SECTION, ASCE, ON TIMBER TEST PROGRAM

COMMITTEE OF THE SAN FRANCISCO (CALIF.) SECTION, ASCE, ON TIMBER TEST PROGRAM: HAROLD B. HAMMILL,[34] M. ASCE, CHAIRMAN, JOHN J. GOULD,[35] HENRY D. DEWELL,[36] AND LEON NISHKIAN,[37] MEMBERS, ASCE, AND HENRY J. DEGENKOLB,[38] ASSOC. M. ASCE.—Although the number of discussions of the report of tests of timber structures from the Golden Gate International Exposition at San Francisco, Calif., has not been great, the committee feels gratified with those presented. The committee realizes that the proper study of a report of this character requires great effort before an engineer is prepared to present a discussion.

Since the completion of testing for this report, many more data on plywood have been published by the United States Forest Products Laboratory in Madison, Wis., and the Douglas Fir Plywood Association. It is gratifying to find that the tests are in general agreement with later, more comprehensive tests and data as reported by Mr. Novick. Regarding stud spacings and the need for intermediate blocking as questioned by Professor Hansen and Mr. Novick, the committee's conclusions referred to the use of plywood in stud walls for resistance to lateral forces and not to glued plywood web girders. Obviously, these conclusions would be open to question when applied to glued plywood webs, which are usually stressed more highly in shear.

NOTE.—This paper by a Committee of the San Francisco (Calif.) Section, ASCE, on Timber Test Program was published in May, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1947, by James H. Carr, Jr., Howard J. Hansen, and E. G. Stern; November, 1947, by Melvin W. Jackson, and Charles Mackintosh; January, 1948, by Sidney Novick, Frank J. Hanrahan, and Walter J. Ryan; and March, 1948, by Walter M. Pratt.

[34] Cons. Civ. Engr., San Francisco, Calif.

[35] Cons. Engr., San Francisco, Calif.

[36] Mr. Dewell died on March 20, 1946.

[37] Mr. Nishkian died on June 2, 1947.

[38] Structural Engr., San Francisco, Calif.

In reference to the questions of fabrication raised by Professor Stern, the split rings and shear plates omitted in certain joints and trusses were omitted by error and not by design. Grooves had been made in the timbers in all cases and carelessness on the part of the erection crews was responsible for the omissions.

The thought contained in Mr. Mackintosh's question—"If the effect of the oversized hole is so great where shear plate connectors are used, should not the manufacturer's recommended value be questioned?"—has no connection with the failure of the cantilever frame in the Cavalcade Building. The report states that this frame was very carelessly fabricated and the relation of bolt size to bolt hole was not in accordance with manufacturer's recommended values. In fact, the report shows that, in tests of other specimens, when manufacturer's values were used with proper workmanship, excellent *R*-values were obtained.

Regarding various questions raised by Professor Hansen, the committee comments as follows:

1. In computing the stresses in a truss with large eccentricities, preliminary stresses were calculated assuming concentric joints. Then diagonal member stresses were corrected for the change in slope and the additional leverage caused by the eccentricities, assuming the points of inflection to be at the midpoints. The forces were then checked for balance and further corrections were applied as necessary.

2. The lower chord of truss TM-2 was reinforced with four 3 by 8 pads, boxing the section, twenty $\frac{1}{2}$-in.-round bolts and eight $\frac{3}{8}$-in.-round stitch bolts.

3. In joint J1, it seemed obvious to the investigators, from previous types of tests, that failure would come at a very low load and that the test would not yield any new information. Therefore, the blocking at the sides of the vertical member was added to investigate a new condition. It is felt that this reinforcement greatly increased the capacity of the joint but manifestly did not give a true picture of the failure of the joint as originally constructed.

4. Regarding the testing of joint J2, an inaccurate statement (see heading, "II. Joint Tests: Concentrically Connected Joints—Joint J2") was made. At the next to the last reading for the first series of tests the top plate had moved 0.132 in., and the bottom plate had moved 0.137 in. When retested the next day, the plates moved approximately the same amount—up to the last reading. Then the top plate moved a total of 0.11 in., and the bottom plate took a sudden jump to 0.40 in. On disassembly it was found that the shear plate on the bottom side had cracked and broken, thus explaining the large deformation.

5. For joints J2 to J17, inclusive, it is correct to state that "* * * in these tests the load was always parallel to grain."

Mr. Carr disagrees with several statements upon which the committee comments as follows:

The *R*-Values Used in the Report.—In many cases where an *R*-value is given for a member that did not fail, data were furnished to show that the element in question successfully held at least the loads shown. Where *R*-values

were so given, the fact was clearly stated. In other cases, for example, truss TNS (see Table 6), all factors were given, as there is unquestionably a doubt as to which was the critical one. In such cases, the highest factor was recorded and used in the tables and discussions. In any case, the comment that the "* * * factor given represents only the minimum amount of test load * * *" is not justified by the facts.

The Use of 1939 Connector Values.—In reference to Mr. Carr's comment on the use of 1935 or 1943 connector values versus 1939 connector values, the committee may be subject to some criticism for using the 1939 values instead of the 1935 values, but certainly not for failing to use the 1943 values. This subject was discussed at great length and the committee decided the report would be more valuable if based on currently used connector values. The committee still considers the decision sound although it was known that new connector values were to be published in 1943. With the tests based on a definite table of connector values, any interested designer can determine, from the information given in the report, the factors found by use of any different table of connector values. Great difficulties were encountered in the preparation of this report and the major part of the computations was completed before publication of the 1943 connector values. The committee believes that the report could never have been published if new computations had been required for results based on the 1943 table of connector values.

It should be mentioned that the major difference in *R*-values between 1943 and 1939 (see Table 27) is found in the 20% stress reduction for members fabricated green and used after seasoning. A more realistic comparison of the basic revisions is given by Professor Hansen in Table 28, which the committee, in general, checks. Although, in Table 28, the effects of moisture conditions when fabricated were neglected, it should be noted that few, if any, improvements of original inconsistencies were made. In fact, when adding all the pluses and minuses, the improvement is +0.41 for seventeen joints, or an average of +0.02 per joint. Furthermore, this improvement is indiscriminate and does not increase low factors nor decrease high ones. Any other improvement is due to the blanket 20% seasoning factor.

If other parts of the test results were compared with the latest design specifications,[9] all tests on bolts would show a 150% increase in *R*-values to account for the presently recommended 67% reduction for fabrication when green, as illustrated in Table 31.

Both Messrs. Carr and Hanrahan have mentioned the new "National Design Specification"[9] and advocate its use, although Table 31 lists large *R*-values. The use of a specification requiring such large *R*-values does not result in sound economic designs. With the exception of joint UI of truss TSF (design and fabrication mistakes are detailed in the report), these bolted joints gave some of the highest *R*-values of the whole test program. Nevertheless, in accordance with new design standards, the factors for these bolted joints fabricated green and tested dry are increased 150%, whereas similar connector joints are increased 25%, although some connector joints loaded at certain angles to the

[9] "National Design Specification for Stress-Grade Lumber and Its Fastenings," National Lumber Mfrs. Assn., Washington, D. C., 1944.

grain proved to be deficient. Certainly, with such a large variation in R-values, further revision should be investigated in the new "National Design Specification" for bolts, and similar revisions should be considered for connectors with parallel-to-grain loading.

TABLE 31.—R-VALUES OF CERTAIN BOLTED CONNECTIONS

Designation	Ratio, R_{bs}		Designation	Ratio, R_{bs}	
	Test[a] report	National design specifications[b]		Test[a] report	National design specifications[b]
(1)	(2)	(3)	(1)	(2)	(3)
(a) Trusses			(b) Joints		
TCh-1	5.3	13.2	J18	8.95	22.4
TCh-2	6.4[c]	16.0	J29	6.31	15.8
TCh-3	5.7	14.2	J30	6.57	16.4
TCh-4	5.9	14.8	J31	6.31	15.8
TCh-5	7.3	18.2	J32	5.54	13.8
	2.94	7.38	J33	7.3	18.2
TSF	2.0[d]	5.0	J34	5.36	13.4
	5.1	12.9	J37	3.03	7.60
	4.65	11.6	J38	3.31	8.3
			J39	5.9	14.7
			J40	5.1	12.7
				4.3	10.7
			J41	7.0	17.5

[a] Based on the recommendations of the Forest Products Laboratory, Madison, Wis. [b] Based on "National Design Specification for Stress-Grade Lumber and Its Fastenings," National Lumber Mfrs. Assn., Washington, D. C., 1944. [c] Bolts. [d] At joint U1.

Tension Across Grain.—Mr. Carr received the preliminary report in progressive stages and therefore knows that considerable space was given to discussing the tension-across-grain features of certain failures and that a design method was tentatively advanced. This method was supported by certain tests,[39] which were made by the University of California at Berkeley on specimens furnished by the Timber Test Committee. The final edition of the report had to be drastically shortened and as a result the "tension-across-grain" method was deleted. The following is quoted from Mr. Carr's discussion:

> "The authors, while stressing the 'tension perpendicular to grain' theory, offer no design approach. Mr. Lank has subsequently been proved correct by a series of tests[8] substantiating the shear theory, and an empirical formula for design application based on this theory has been developed * * *"

Everett Lank's formula has been developed from tests of fifty-four small-size, dry, selected laboratory specimens with results varying more than 100%. From experience on the timber test program the committee is convinced of the soundness of the conclusion (see heading, "A. Foreword"), which calls atten-

[39] "Tests and Analysis of Crossgrain Tension in Eccentric Wooden Truss Joints," by O. H. Degenkolb and J. J. Holstein, a thesis presented to the University of California at Berkeley, Calif., in 1942, in partial fulfilment of the requirements for the degree of Bachelor of Science.

[8] "Tests of Eccentric Split-Ring Joints Yield Rational Design Formula," by R. H. Glass, *Engineering News-Record*, May 29, 1947, pp. 69–70.

tion to "1. The inadequacy of small individual tests as a premise for determining the behavior and strength of full-sized structures." The committee believes that conclusions from such small-size tests should not be considered in preparing specifications for universal use until the results of such tests have been checked by testing full-sized specimens.

With regard to factors of safety, it is realized by the committee that, in testing complete structures or trusses, smaller factors are acceptable than for individual joints. The committee does not disagree with a factor of 2 as noted in the last paragraph of Mr. Carr's discussion, provided all factors of design are considered, including long-time loading.

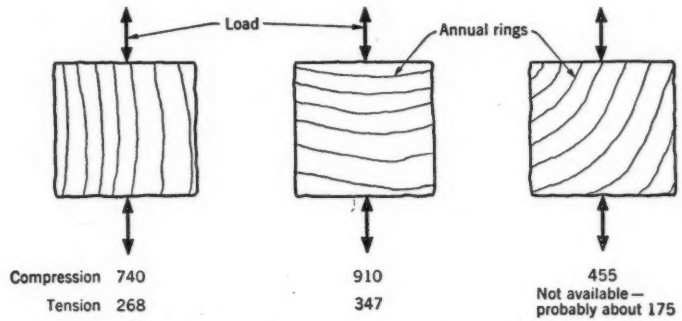

FIG. 90.—ULTIMATE STRENGTHS, IN POUNDS PER SQUARE INCH, OF DOUGLAS FIR AT 12% MOISTURE CONTENT LOADED PERPENDICULAR TO THE GRAIN AND AT VARIOUS ANGLES TO THE DIRECTION OF ANNUAL RINGS

The committee is very much interested in the results found by Messrs. Jackson and Mackintosh on their joint tests. Fig. 87 confirms the committee's opinion that the stresses in a typical timber joint using split rings are definitely connected with the tension perpendicular to the grain strength of wood. Particular notice has been paid to the variation of the tension strength from 90 lb per sq in. to 380 lb per sq in., as this confirms the committee's observations. It is believed that the worst condition for tension across the grain is not usually reported (see Fig. 90). For example, the "Wood Handbook"[2] gives average values of 740 lb per sq in., 910 lb per sq in., and 455 lb per sq in. for the compressive strength perpendicular to grain of wood when load is applied parallel, perpendicular, and at 45°, respectively, to the annual rings. The Forest Products Laboratory in unpublished data also gives 268 lb per sq in. and 347 lb per sq in. as the average tensile strengths when load is applied parallel and perpendicular to the annual rings, but neither knowledge of tests nor test data are available for conditions at 45°. However, by analogy to the compression tests, the 45° condition would possibly be the minimum, with an average value of about 175 lb per sq in. Considering variability and other factors, this could help explain many erratic results. In the University of California[39] tests, it was inferred that this tension strength at 45° might be related to the number of annual rings per inch; the closer the annual rings,

[2] "Wood Handbook," Forest Products Laboratory, Forest Service, U. S. D. A., Madison, Wis., 1940.

the higher the tension strength. In the joint shown in Fig. 86, it is felt that it would have been better if the horizontal members A and B had been longer, since the University of California tests confirm the theoretical distribution of tension stress as extending each side of the point of application of the force about two and one-half times the depth of the member. Thus, for example, for a 2 by 6 member the minimum length is 5 × 6 in., or about 30 in., and for a 2 by 12 member, it is about 5 ft. The effective length of tension surface, from both tests and analyses, seems to be about 3.1 times the depth of member.

The Douglas Fir Plywood Association reports that three of the four subjects recommended by the committee for further consideration have already been considered. The general case of plywood buckling, together with specific applications, has been solved both theoretically and experimentally by the Forest Products Laboratory.[26] The Treasure Island tests confirm the formulas of the Forest Products Laboratory.

The lateral bearing strength of nails having reduced edge distances in plywood is discussed, and test data are presented in Section 6 of "Technical Data on Plywood."[40]

Some information on the behavior of large plywood sheathed walls, both with and without openings, under wracking loads is contained in an article by Alan D. Freas,[41] Assoc. M. ASCE. Further test data on the shear strengths of specific prefabricated plywood panels may be found in various reports of the National Bureau of Standards.

In Table 28, Professor Hansen gives values of R on the ultimate shear loads of Douglas Fir Plywood shear tests. It is unlikely that the grades tested were higher than sound two sides, according to officials of the Douglas Fir Plywood Association who examined these panels for the committee. The allowable stresses used are evidently based on stresses recommended for loads at 45° to the grain, which are twice those recommended for parallel and perpendicular directions. N. S. Perkins and D. Countryman of the Douglas Fir Plywood Association, in a communication to the committee, state that in their opinion these R-values should be based upon the parallel condition, which would give double the values. The tests were made with plywood in "pure" shear. Neglecting edge conditions, shear is equal in all directions. The committee presents this information without comment.

The committee appreciates the favorable comments contained in the discussions of Messrs. Pratt and Ryan, and the helpful suggestions contained in correspondence from Messrs. Perkins and Countryman of the Douglas Fir Plywood Association.

The committee deeply regrets that the following engineers who contributed so much to the development of this report did not live to see it published: Henry D. Dewell and Leon H. Nishkian, Members, ASCE, members of the committee; Chester J. Hogue, M. ASCE, of the West Coast Lumberman's

[26] "Design of Wood Aircraft Structures," *Bulletin No. 18*, Army-Navy-Commerce Committee on Aircraft Requirements, U. S. Govt. Printing Office, Washington, D. C., 1942.

[40] "Technical Data on Plywood," Douglas Fir Plywood Assn., Tacoma, Wash.

[41] "Guides to Improved Framed Walls for Houses," by Alan D. Freas, *Engineering News-Record*, October 17, 1946, p. 117.

Association; and J. E. Mackie, Assoc. M. ASCE, of the National Lumber Manufacturers' Association.

Corrections for *Transactions*: In May, 1947, *Proceedings*, on pages 603 and 610, interchange the figure numbers and titles of Figs. 20 and 25. Beginning with Table 26 in October, *Proceedings*, all table numbers and references to them should be advanced by one, thus: Table 26 should read Table 27, etc. (The tables mentioned in the following sentences have corrected numbers.) In October, 1947, *Proceedings*, on page 1324, Table 27(*b*), change 4.45 to 2.56; Table 27(*d*), joint L3, change 3.15 to 2.43; Table 27(*e*), joint J11, change 5.70 to 3.58; Table 27(*f*), joint J1, change 3.56 to 3.12; and delete the paragraph beginning "The writer cannot reconcile * * *" and insert "Applying the shear formula to the French Building truss at failure, the 1943 factor becomes 2.56 (Table 27(*b*)) as against the 1939 factor reported, based on connector values (which did not fail) of 1.7, an appreciable increase." Also, in October, 1947, *Proceedings*, on page 1325, lines 11 and 31, change 2.98 to 2.43; on page 1326, Table 28, joint J8, change 3.37 to 4.47 and −1.40 to −0.30; and, on page 1328, delete the last paragraph.

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DISCUSSIONS

LEAST SQUARES ADJUSTMENT OF TRIANGULATION NET BETWEEN GEODETIC STATIONS

Discussion

BY E. F. CODDINGTON AND O. C. J. MARSHALL

E. F. CODDINGTON[14] AND O. C. J. MARSHALL,[15] Members, ASCE.—The discussions express three general opinions concerning this paper: First, the need and timeliness of a simple method to fill in the 25-mile gaps of the first order triangulation nets provided by the United States Coast and Geodetic Survey (U. S. C. and G. S.); second, an excellent description of this control net by Mr. Hemple; and, third, a questioning by some of the discussers of the accuracy of the method.

Messrs. Ney and Simmons raise the question of the accuracy of the method, especially when extending basic control nets. Mr. Ney demonstrates that the computation errors were insignificant in the case of the synthetic net used by the writers. The Logan County (Ohio) net is smaller than the synthetic net. As to the question of rigid or correct theory raised by Mr. Simmons, it may be stated that practically all geodetic theory is applied by a succession of approximations. The number of significant terms in the series of approximations increases with the length of lines or with the size of area under consideration. The writers have been successful in applying mechanical integration to find accurate lengths of elliptical arcs on the earth's surface.

The writers are convinced that local rectangular coordinates are sufficiently accurate for areas under 25 miles square, or to fill in control in any 25-mile gap in the completed first order triangulation net executed by the U. S. C. and G. S. The local rectangular coordinates of all geodetic stations of the net adjusted to the first order control may be easily transferred back to geographic coordinates (latitude and longitude) in case state-wide coordinates may be desired for mapping purposes. This method was derived to enable city engineers to use local coordinates in making cadastral surveys tied to first order geodetic control.

NOTE.—This paper by E. F. Coddington and O. C. J. Marshall was published in September, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1948, by Julius L. Speert, C. H. Ney, H. W. Hemple, and Lansing G. Simmons.

[14] Prof. Emeritus of Geodetic Eng., Ohio State Univ., Columbus, Ohio.

[15] Prof. of Surveying and Geodesy, Univ. of Toronto, Toronto, Ont., Canada.

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DISCUSSIONS

INFLUENCE LINES FOR CONTINUOUS STRUCTURES BY GEOMETRICAL COMPUTATION

Discussion

BY DEAN F. PETERSON, JR.

DEAN F. PETERSON, JR.,[22] ASSOC. M. ASCE.—Mr. Spaulding's criticism that the fundamental equation of flexure, $\frac{d^2y}{dx^2} = \frac{M}{E\,I}$, does not apply to curved members is a criticism of all the modern methods for analyzing stresses in frames—from the classical theorems of strain energy to moment distribution and the elastic curve traverse. All common methods of frame analysis are based on Eq. 1 and give identical results. A discussion of the shortcomings of the basic assumptions of these methods is beyond the scope of this paper. Part of the writer's purpose was to demonstrate how to utilize these methods of solving for the end rotations and deflections of the members of unloaded frames subjected to the proper relative deflections to produce the desired influence line. The basic thesis of the paper will not be amended by greater refinement in the application or the premises of these methods.

Considerable progress has already been made toward understanding the action of knees and curved members subjected to flexure. The work of Bruce Johnston, M. ASCE and Edward H. Mount[23] and a paper by Mr. Stewart[24] are valuable contributions toward explaining the action of structural joints, including knees in framed structures. Such amendment in procedure as may be necessary is easily accomplished by interpretation and clarification of Eq. 1. The action of curved beams is treated by standard texts on strength of materials and unless the curvature is relatively great—that is, unless the radius of curvature approaches the depth of the beam—the corrections to Eq. 1 are immaterial.

NOTE.—This paper by Dean F. Peterson, Jr., was published in September, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1947, by Ralph E. Spaulding; and May, 1948, by Ralph W. Stewart, Yi-Mai Yao, and Otto Gottschalk.

[22] Associate Prof. of Civ. Eng., Utah State Agri. College, Logan, Utah.

[23] "Analysis of Building Frames with Semi-Rigid Connections," by Bruce Johnston and Edward H. Mount, *Transactions*, ASCE, Vol. 107, 1942, p. 993.

[24] "Analysis of Frames with Elastic Joints," by Ralph W. Stewart, *Proceedings*, ASCE, December, 1947, p. 1499.

Eq. 1 is also used by the writer for determining the ordinates to the elastic curves of the various members of the frame. Curvature of the members of frames is so small that corrections to Eq. 1 when thus used become unnecessary.

The contribution by Mr. Yao is valuable in that it demonstrates an additional method for finding the end rotations of the members of the unloaded structure subjected to the desired strain conditions.

Mr. Gottschalk criticizes the writer's use of nongeometrical methods for determining the end rotations of the various members of the structure bearing the imposed strain conditions. The writer claims to have used geometrical methods only for computing the deflection curve after the end rotations have been determined. Calculation of the end rotations of the members can be made by any method the designer desires. Whether he uses methods based principally on geometry, such as the elastic curve traverse, or the method proposed by Mr. Gottschalk (Table 11) is immaterial to this paper. Examples are given using several methods, although the writer's personal preference is for the geometrical methods.

The writer agrees with Mr. Gottschalk that modern structural theory is cluttered with unnecessary symbolism. It is more fundamental and just as easy, for example, to consider a change in the inclination of the tangents at two points of a beam as an angle (which it really is), instead of as the shear or weight borne by an imaginary beam. On the other hand, the writer believes a realization, for instance, of the relationship between bending moments and beam curvature to be essential. Use of real bending moments to express curvature is not believed to be "abstract symbolism."

Mr. Gottschalk states that, for prismatic beams, "* * * the laborious work of computing the tables required by the author is made unnecessary," and suggests Eq. 25. Of the seven tables, six concern computation for nonprismatic members. In all examples for prismatic members, the writer used Eq. 10 which is a generalized statement of Eq. 25.

Table 11 shows a method for obtaining the distribution factors on both sides of the support of a continuous beam. This method is given as a substitute for moment distribution and is therefore interesting. Mr. Gottschalk does not make clear, however, the method for finding the distribution of an unbalanced rotation at a remote joint—for example, the distribution at point D resulting from a rotation at point C. Moment distribution is only one of the many methods by which rotations over the supports of the continuous beam with unit rotational strain imposed at point B (Fig. 11) may be deduced. These may be found with great rapidity, for example, using the elastic curve traverse.[25] The method to be used depends somewhat on the personal preference and training of the designer.

In conclusion, the writer believes that the influence line computed as the deflection curve of the unloaded structure subjected to the proper strain conditions furnishes the simplest method of analyzing the stress condition of a structure if more than one loading is to be considered. The work involved in solving for the influence line in the manner proposed is about equal to that involved in a stress solution for any single set of loads.

[25] Discussion by Dean F. Peterson, Jr., of "Flexure Factors for Analyzing Continuous Structures," by Ralph W. Stewart, *Transactions*, ASCE, Vol. 104, 1939, p. 533.

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DISCUSSIONS

APPLICATION OF GEOLOGY TO TUNNELING PROBLEMS

Discussion

BY ERNEST E. WAHLSTROM

ERNEST E. WAHLSTROM,[21] ESQ.—The carefully considered discussion by several well-qualified experts emphasizes the diversity of opinion as to the exact functions of the geologist on engineering projects. This is only natural because geologists serve in different capacities from one project to another and have been assigned differing degrees of responsibility by engineering firms and governmental agencies. Moreover, the discussion clearly demonstrates the difficulty of writing a paper that places emphasis on general principles rather than on specific examples. Generalizations are always subject to exceptions.

Geology is a very complex, fluid science, and a geological paper written in a condensed, more or less academic manner should not be construed by the reader to mean that geologists work by rule of thumb or make interpretations which are based on elementary considerations only. Actually, a successful engineering geologist is a scientist who interprets geologic and engineering data artfully in the light of knowledge gained from long years of rigorous academic training and diversified field and laboratory experience. The engineering geologist attempts to apply the complex science of geology to the equally complex problems of engineering. He should remain primarily a geologist and should not attempt to usurp the place of the engineer, and first and last should think and act as a geologist and not as an engineer with a smattering of geological training.

There is an urgent need for publications that clearly detail and discuss "case histories." The vast amount of data in files of engineering firms and governmental agencies should be collated and made available to the geological and engineering professions as expediently as possible. Free interchange of ideas and widespread dissemination of the published results of geologic investigations of engineering projects will enable the practicing professional man measurably to improve both the qualitative and quantitative aspects of his work.

NOTE.—This paper by Ernest E. Wahlstrom appeared in October, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1948, by Berlen C. Moneymaker, and Robert S. Mayo; March, 1948, by F. A. Nickell, R. H. Keays, and Jacob Feld; and May, 1948, by Thomas W. Fluhr, Portland P. Fox, Hyde Forbes, and Roger Rhoades, A. B. Reeves, and W. H. Irwin.

[21] Dept. of Geology, Univ. of Colorado, Boulder, Colo.

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DISCUSSIONS

THE GENERAL PRINCIPLES OF HIGHWAY TRANSITION CURVE DESIGN

Discussion

BY JOHN JOSEPH LEEMING

JOHN JOSEPH LEEMING,[34] ESQ.—It is gratifying to find so much agreement with the principles proposed in the paper, in spite of the fact that the proportion rule does not find general acceptance. Indeed, the writer must admit that he is not fully satisfied with the rule in its present form. It is probably too rigid, and tends to give excessively long transitions on the longer curves. Since the paper was written, the work of the experiments has been concluded and the statistical analysis is in progress, although it will be some time before the latter will be complete. It seems possible that some sliding scale of reduced proportion of transition for longer curves may result, although this is only conjecture. For the moment, the writer is inclined to agree with Mr. Haile that the rule should only apply to short curves. In spite of its shortcomings, however, the rule has some experimental background, and does represent some attempt to relate transition curve design to facts. The writer cannot agree with Mr. Haile that there is "insufficient justification for changing current practice," since the latter has no theoretical or experimental justification at all.

Messrs. Noble and Fisher both object to the proportion rule on the ground that it involves spirals with different rates of curvature for different radii of curves. Nevertheless, it appears from the experiments that drivers do vary the scale of the transitions they use. This is the meaning of the observed variable Q, since Q is a measure of the scale of the spiral. To demand a constant shape of spiral, as Mr. Fisher does, is to return to the principle of the constant C, which the writer has shown to be unsound.

Professor Meyer and others desire further particulars of the experimental technique. Present space hardly permits this; but the original paper has been deposited in the Engineering Societies' Library.[34a] In reply to Mr. Noble, however, the writer did read the report on the high speed tests on the Penn-

NOTE.—This paper by John Joseph Leeming was published in October, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1948, by Carl F. Meyer; and May, 1948, by Ralph L. Fisher, William R. Welty, Elmer R. Haile, Jr., and Charles M. Noble.

[34] County Surveyor, Dorsetshire, England.

[34a] 29 West 39th St., New York 18, N. Y.

sylvania Turnpike,[21] and his study of the report influenced his own experiments somewhat. The latter, however, were intended to deal with ordinary touring conditions and with the ordinary driver, whereas the Pennsylvania tests were designedly carried out at extreme speeds with highly skilled drivers, and were mainly directed toward finding the maximum safe value of j at high speeds.

It is not possible to state what proportion of the curves analyzed had transitions. In the first stage of the work the mean proportions of transition, for various classes of roads worked out as follows:

Type of road	Average value of P	Number of curves
First class	0.64	507
Second class	0.67	379
Unclassified	0.67	370
Rotary intersections	0.64	43
Right-angle junctions	0.67	38
Total		1,337

Values are not available for the total number of curves analyzed. The first class roads included a fair proportion of curves with transitions (although some of these were of compound circular arcs) and superelevation; the second class, a few; and the remainder, probably none at all. The experimental results give no reason to believe that the presence of a transition either makes for any greater consistency on the part of the driver, or prevents him from cutting corners. As a result of his observations, the writer is convinced that the latter process is due to the principle stated by Mr. Haile (and with which the writer is in complete agreement) that "* * * the driver guides his vehicle by the appearance of the road many car lengths ahead," combined with the geometrical properties of the compound curve as shown in Fig. 3. Ordinarily, the driver does not begin to turn before he reaches the start of the curve. Whenever the records could be compared with the road conditions, the length of the curve on the record was not appreciably greater than the corresponding length of the curve on the ground, and often, it was shorter. If, then, the driver begins to turn at about the start of the spiral, the quicker he turns his wheel, the flatter will be the curve he follows and the less will be the value of j (that is, the force he has to apply at the wheel), although the throw or cut will be greater. Since he has no objection to a high value of Q, and since he is, in Mr. Welty's words, "humanly lazy," the driver takes the line of least resistance and cuts the curve if its general appearance leads him to believe that it would be feasible. This will usually be the case if a sharp curve has unduly long transitions. On the other hand, short transitions anticipate the cut, as it were, and lead to as flat a curve as the conditions allow. A high value of Q does not increase the driver's work.

The foregoing, however, is not a complete explanation, as the following example shows. A careful analysis was made of an all-transitional curve, laid

[21] "Curve Design and Tests on Pennsylvania Turnpike," by Kenneth A. Stonex and Charles M. Noble, *Proceedings*, Highway Research Board, National Research Council, Washington, D. C., Vol. 20, 1940, p. 429.

out and constructed with great care early in World War II. A diagram of the curve is shown in Fig. 9 and the design data for the lemniscate curves of the inner and outer edges are given in Table 5. The deviation angle, I, of the entire curve is 39° 26′; the hands-off speed, 22.5 miles per hr; and the superelevation at the apex, 0.07. The curve is admittedly sharp, especially so, as it was on an important road which carried much traffic. It was located at

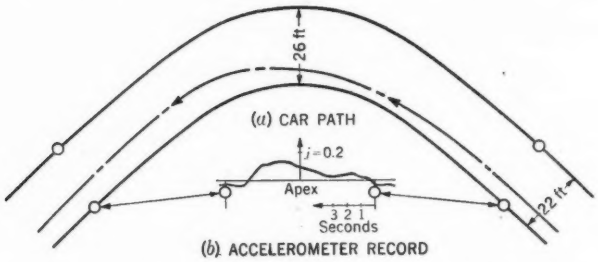

FIG. 9.—TYPICAL RECORD OF CAR ACTION ON ALL-TRANSITIONAL CURVE AT 39 MILES PER HR

the start of a diversion of an originally straight road, built for the construction of an aerodrome, but owing to wartime conditions there was only a minimum of construction. Recordings were made with the accelerometer, tied in with the tangent points of the curve, and checked by timing. Other traffic was observed and timed. A typical record, taken at 39 miles per hr, is shown in

TABLE 5.—DESIGN DATA FOR LEMNISCATE CURVES IN FIG. 9

Description	Outer curve (ft)	Inner curve (ft)
External distance, E	32.2	35.0
Tangent distance, T	297.6	323.5
Half length of curve, $\frac{L}{2}$	290.2	315.7
Minimum radius, R	456.5	496.0

Fig. 9, in which it can be seen that the record is markedly unsymmetrical. The car's probable path is shown, and the observations are confirmed that most of the faster traffic followed a similar line. (The English left-hand rule of the road must be remembered in interpreting Fig. 9.) In this case, the most probable explanation is that the driver follows the first half of the curve fairly closely, although some slight tendency to cut the apex is possible. When he reaches the apex, the sharpening of the curve passes unnoticed until the swerve at the three-quarters point becomes obvious, and a sharp exit is made. It was also noticed that traffic in the opposite direction cut the curve very noticeably, yet in this case the recordings were of a more normal type.

In reply to the questions raised by Mr. Haile, the analysis of comfort is not yet complete, as it involves a statistical analysis of great complexity. Provisionally, it may be stated that about half the observers found the value of $j = 0.13$ combined with the value of $Q = 4$ ft per sec^3 to be imperceptible. A similar number of observers thought $j = 0.22$ in combination with $Q = 5$ ft

per sec^3 to be uncomfortable. In the later work, no distinction was made between the two classes of discomfort. So far, it has been impossible to state whether speed has any effect on the estimation of comfort. Body roll is included in these values of j, and, since it is one of the sensations of the driver, it should be included in analyzing comfort. Mr. Noble[33] found that this effect would amount to about 10% of the real value of j. British cars tend to be more stiffly sprung than American cars, and their body roll might be less than this.

Mr. Welty's experiments were unknown to the writer, and are of interest in that they agree so well with his own conclusions, even though the value of Q found was higher than that of the writer and Mr. Black. Derivation of Q by the analysis of the car path by any graphical method is probably less accurate than by the direct accelerometer method. The writer does not think that the accelerometer unduly influenced the drivers. He himself was one of them, and he found that he soon became accustomed to the presence of the instrument and paid no further attention to it. Some deliberate swerves were made for study, and recorded, but the results were not used in the analysis.

With reference to Mr. Fisher's contribution, the use of the term "friction factor" was deliberately avoided although the writer was aware that it had been used in the United States. The term "lateral ratio" was adopted after careful thought, because he found a frequent tendency for confusion, both in the literature on the subject and in discussions with his colleagues, between the friction factor and the coefficient of friction as understood in dynamics.

If the writer understands Mr. Noble correctly, the method of transition design, mentioned by him as proposed by Mr. Haile,[30] and by T. T. Wiley,[31] Assoc. M. ASCE, which determines the length of the curve "* * * on the basis of rotational change in attaining the curve superelevation," is open to the same theoretical objection as the method of W. H. Shortt. Referring to Eq. 1, Mr. Shortt based the length of transition on C, which is proportional to $\frac{d}{dt}\left(\frac{v^2}{R}\right)$, disregarding the superelevation. Those of Messrs. Haile and Wiley, on the other hand, are apparently based on $\frac{d}{dt}(\sin \alpha)$, neglecting C. Both should be considered, if the rate of change of force is a criterion. In his early work the writer derived a formula for the length of transition based on Q, and was faced by the difficulty, mentioned by Mr. Haile, that, when j is zero (that is, at the hands-off speed), the length of transition becomes zero. This result is nonsense, yet it is logically demanded by the theory. The fallacy was suspected to lay in the use of the rate of change of force, and the experiments confirmed the suspicion. It is true that very short transitions may produce a sharp rise in the superelevation, but this rise does not effect the rate of change of force. Any unpleasant effect from the rise is very possibly due to the psychological effect of a sudden rotation of the horizon. There is also the question of the unsightliness of a sharp rise in the curb or in the edge of the roadway.

[33] "Developments in Curve Design, Speed and Sight Distance," by Charles M. Noble, *Roads and Streets*, January, 1942, p. 25.

[30] *Transactions*, ASCE, Vol. 102, 1937, p. 1097.

[31] *Ibid.*, p. 1105.

The writer must dispute Professor Meyer's claim that the Shortt formula $\left(L = 1.6\frac{v^3}{R} \text{ if } C = 2\right)$ makes L dependent on superelevation. Mr. Shortt defined C as the rate of change of centrifugal force. The only other terms are v and R, and there is no term which could be said to include, in any way, superelevation. The fact that the latter may be based in some way on R does not affect the formula, which gives the same length of curve whatever the relation between superelevation and R.

In conclusion, the writer strongly supports Mr. Noble's plea for adequate research, and hopes that this paper will help in achieving that end.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

DRAINAGE OF AIRPORT SURFACES—SOME BASIC DESIGN CONSIDERATIONS

Discussion

BY STIFEL W. JENS

STIFEL W. JENS,[30] M. ASCE.—Messrs. Williams, Hicks, and Izzard all question the validity of the technique involved in processing the rainfall data. Mr. Williams doubts the need for establishing a storm pattern, and recites some of the reasons why he believes the considerable effort involved in setting up a correct storm pattern is not warranted. Acknowledging the reasonableness of these criticisms, the method of the paper does approach the realities whereas the more usual "rational method" techniques, which utilize one average intensity only, clearly do not. There can be no denial that the amount of detailed work is likely to discourage its use, and Mr. Williams' constructive suggestion for a simpler approach is very interesting. The striking similarity of the storm patterns for the widely separated geographical locations is encouraging. Use of the method illustrated by Tables 10 and 11 should recognize, however, that a pattern made up entirely of intensities taken from the ordinary intensity-duration curve, rearranged in an order suggested by a brief analysis of the average pattern of a number of recorded storms, is an almost impossible combination of events. Certainly such a storm pattern departs more from the statistically valid pattern than do those developed as suggested in the paper.

The user of the Williams method should also keep in mind the fact that a pattern rainfall incorporating all the intensities for the various durations for one frequency, necessarily will result in a generally higher net rainfall pattern, because the infiltration capacity will be equaled or exceeded earlier and oftener. A correct pattern of net supply would undoubtedly have more intervals of rainfall intensities lower than the related infiltration capacities.

The incidental illustration of the usefulness of the Hathaway standard rainfall intensity-duration curves points to a possible utilization of these for areas in which no satisfactory local record is available. To the writer, the Yarnell[13]

NOTE.—This paper by Stifel W. Jens was published in September, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: and May, 1948, by H. M. Williams, W. I. Hicks, E. J. A. Gain, E. F. Brater, and Carl F. Izzard.

[30] Partner, Horner and Shifrin, St. Louis, Mo.

[13] "Rainfall-Intensity-Frequency Data," by D. L. Yarnell, *Miscellaneous Publication No. 204*, U. S. D. A., Washington, D. C., August, 1935.

curves are preferable, if used carefully with an understanding of their limitations. The supposition that the paper uses the method of G. A. Hathaway, M. ASCE, with some modifications is correct, but the background and initial application of the infiltration-overland flow technique were originally developed by W. W. Horner, Past-President, ASCE, in the studies made for the drainage of the Washington (D. C.) National Airport in 1939.

Mr. Hicks calls attention to the fact that only one third of the twelve storms of Table 1 have net rainfalls of 20-min duration; of the remaining eight, two have 15-min net rainfalls, four have 10-min supplies, and two sustain a supply rate for only 5 min. The wide variation in outflow rates for each of these different net supply rates leads Mr. Hicks to the sound conclusion that the intensity-duration curve should be based on the duration and intensity of net rainfall. The objective of the designer is to know, for any specific part of a drainage system, the frequency with which certain rates of flow will be equaled or exceeded. Obviously, if the true frequency of supply rates of different durations could be determined, the foregoing objective would be immeasurably closer. Lack of sufficient data, together with the need to recognize specifically, and in some detail, more of the many factors influencing net rainfall has made common the use of the frequency of gross rainfalls of various durations.

The probable gross rainfall pattern of a desired frequency is applicable to a sizable area. However, a different net rainfall pattern is required for each area with a different soil-cover-condition complex. Different soil, cover (and stage of growth), antecedent moisture, and season, to mention but a few of the many factors involved, each affect the net supply. The probabilities are that gross rainfall frequencies will continue for some time to come to be the guiding criteria in so far as frequency enters into storm sewer design. The important practical aspect of this fact is always to keep in mind the often considerable difference between the frequency of a given gross rainfall and the frequency of the runoff derived therefrom. Judgment will be better guided in the light of such recognition of the realities.

Mr. Izzard's illustration of the impossibility of an arithmetic mean actually occurring under certain circumstances does not seem to the writer to be altogether comparable to the determination of an average storm pattern. The desired objective was to evaluate the effects, on the critical period net rainfall, of immediately antecedent precipitation as it might occur on the average during the storm frequency under study. In the light of the almost infinite variety of patterns of rainfall intensities, it does not appear likely that a statistical study would reveal a storm that occurred on the average once every one, two, five, or ten years and actually repeated itself in every detail the number of times implicit in its indicated frequency. The method of developing average supply patterns, as illustrated in Table 1, is open to criticism of its statistical validity, but nonetheless it is believed to be a reasonable attempt to give quantitative expression to some of the actual happenings which are qualitatively known.

Professor Brater's review of the development of the Horton overland flow formula (Eqs. 2 and 24) and his analysis of its limitations are particularly convenient for comparison with Mr. Izzard's dimensionless hydrograph of overland flow. It is interesting to note that the latter's formula evaluates some of

the factors Professor Brater lists as not reflected in the Horton equation. The additional retardance to sheet flow due to raindrop impact is recognized in Eq. 31 by the factor $0.0007\ \sigma$. The "effect on the velocity distribution * * * by * * * infiltration of the low velocity water in contact with the ground surface" is reflected indirectly in both Eqs. 2 and 7 in that the net supply rate is used and retention or depression storage and infiltration have been abstracted from gross rainfall.

Although Eq. 25 attempts to set up a mathematical expression for the variation of supply with time, practical application of either the Horton equation or the Izzard equation has involved the use of a stepped pattern net rainfall, each step considered to have a uniform rate over its short time. Mr. Izzard has indicated the principle on which both his and Mr. Horton's equations can be used to reflect changing rates of supply—namely, that the rate of runoff is directly related to the amount of detention at any time.

Both overland flow equations are admittedly only valid for the rising side of the hydrograph. In recognition of this, Mr. Izzard has developed from his experimental work another dimensionless equation[28] to compute rates of runoff during recession.

Professor Brater's illustration (Fig. 16) corroborates Mr. Izzard's experimental findings that for sheet flow, where $i\,L < 500$, the exponent $m = 3$, which indicates that the flow is steady, uniform, and laminar. Everyone interested in this matter awaits with eagerness the findings of the current experimental program of the Corps of Engineers to determine the interrelationships where the flow is fully turbulent. If the comparable curves computed by Eq. 2 with an exponent of 3 instead of 2 are added to Fig. 18, the close agreement of the two sets of curves is a tribute to the sound analytical approach of the late R. E. Horton, M. ASCE, and the careful experimental work of Mr. Izzard. The latter's dimensionless hydrograph for the rising side and his companion dimensionless recession hydrograph are based on analysis of a considerable volume of experimental data and from a practical viewpoint suggest that the theoretical limitations so well discussed by Professor Brater, even if individually serious, are apparently either canceled out in a compensatory fashion, or are properly reflected in the empirical formulas.

No one can quarrel with Professor Brater's preference for "* * * a method in which the effect of the various assumptions is clearly evident." One of the major objectives of the method discussed in this paper is an attempt to reflect more clearly the sequence of events as they occur. The writer prefers the approach involving the use of an overland flow equation such as the Izzard dimensionless hydrograph (simpler in form and easier to use than the cumbersome hyperbolic function of the Horton equation). Had the Izzard overland flow equations been available and understood, at the time of the studies leading to this paper, they would have been used. The very material amount of detailed computation required to change to the Izzard approach militated against a change-over.

[28] "Hydraulics of Runoff from Developed Surfaces," by Carl F. Izzard, *Proceedings*, Highway Research Board, National Research Council, Vol. 26, 1946, pp. 129–150.

The difficulties inherent in obtaining representative discharge storage relationships for both the rising and recession sides of the hydrograph of sheet flow are many and involve, in the writer's opinion, no less departure from the realities than the use of the overland flow equations. Professor Brater suggests that the constants of Eq. 26 "* * * are usually determined from the recession side of a hydrograph" of overland flow. That is exactly what Mr. Izzard has done in establishing the relationship between discharge and detention storage. It is believed that the effect of the sheet flow detention storage on the rates of net rainfall or supply is most practically expressed by an empirical formula. The many factors and their complex interrelationship, so well discussed by Professor Brater, will probably never be expressible in any practical rigorous mathematical form.

Mr. Gain has lent clarity to the paper in his succinct summary of the attempt by the method to follow logically and realistically the development of runoff from rainfall. Even if a workable combination of the more detailed and realistic methods, such as used in the paper, can be effected with the "rational" method, in the long run the more detailed method will be most desirable, because the designer can visualize the process much more clearly as it develops. Of course, it is highly probable that, in the evolution of better founded methods of predicting runoff from precipitation, some such intermediate step, as is suggested by Mr. Gain, may occur.

Although it is true that municipal practice ordinarily can make no use of the rate-reducing storage of deliberate pondage, in many very flat areas of intensive development (such as the wide, alluvial river bottoms at Kansas City, Mo., East St. Louis, Ill., and similar industrial developments along the great rivers of the United States), storm sewer design does not attempt to provide inlet and sewer capacities adequate for the very short times of concentration actually prevailing at upper ends of lines. The very small mass values of net rainfall involved in the very short-time high intensity rainfalls make it economic and practical actually to design upper end inlets and laterals for the lower intensities related to longer durations. In other words, in such circumstances, a minor amount of actual pondage around the inlets prevails for a relatively short period when the storm of design frequency occurs.

Everyone interested in the application of the newer hydrology seconds wholeheartedly Mr. Gain's plea for additional data and research. To cite one of the outstanding examples, the vast amount of experimental data on rainfall, infiltration, and runoff collected by the Soil Conservation Service at its experimental watersheds during the 1930's should be processed and be made available at the earliest possible moment.

The field investigations of airport drainage made by the Corps of Engineers and discussed by Mr. Williams constitute a valuable supplement to the hydrologic information used in design methods such as that suggested by this paper. Wheeled traffic over turfed airport areas should be practically nil on civil fields, but the other difficulties encountered in maintaining constant drainage areas indicate the need for conservative judgment in design. Because of the unavoidably flat ridges and drainage divides, the writer is particularly impressed by the possibility of "* * * wind driving surface water over low

drainage divides." Since nothing practical can be done to prevent this, it can only be recognized through the employment of reasonable safety factors as suggested by Mr. Williams.

If Mr. Izzard's example to illustrate the use of Eq. 32 would use a value of net supply rate of 1.26 in. per hr for the composite area, it would employ the actual supply rate interpolated from Fig. 6 for a 27% impervious area and a 30-min duration of supply. The outflow rate at the lower edge of the composite area, as computed by Eqs. 7, 8, 9, and 10, is 1.06 in. per hr. Examination of Fig. 10 indicates a peak runoff rate of 1.12 in. per hr for 135 ft of turf length and 30% imperviousness—in keeping with Mr. Izzard's close check of the peak runoff rates in Table 2, except for short durations of supply on wholly pervious areas.

The limitations of the triangular hydrograph are brought out by Mr. Hicks' analysis as illustrated by Fig. 15 and by Mr. Williams' citation of the general conclusions arising out of studies of the Airfields Branch, Office of the Chief of Engineers, United States Army. Mr. Izzard indicates that brief examination of actual hydrographs suggests a peaked triangular outflow hydrograph as illustrated in Fig. 19. The criterion of the ratio of available pondage to mass runoff is an excellent guide to the designer. However, all the discussion points to the need for further study and research to develop a practical tool to take the place of the triangular hydrograph and at the same time to lend itself to reasonably accurate evaluation of pondage.

Mr. Hicks shows clearly the error of summating triangular hydrographs where ponding is not present. With the Izzard dimensionless hydrograph, actual inflow rate curves can be developed readily and then offset for addition. Mr. Hicks' discussion of Fig. 15 shows the small change in pondage depth necessary for an appreciable difference in required pondage volume. This fact is fortuitous. Examination of the dimensionless hydrograph supports Mr. Hicks' statement that, up to a duration of about 0.6 of the equilibrium time, the triangular hydrograph is overly safe; as it passes through that point and continues on to equilibrium, the straight ascending leg of the triangle falls below the curve values. The differences are not great until the duration involves a prolonged flow at equilibrium rate.

The writer cannot agree with Mr. Izzard's assumption that "* * * ponding begins at the beginning of the inflow hydrograph." Until the overland flow rate into the collecting inlet exceeds the capacity of the outlet, no pondage could result. One other assumption involved in the method illustrated by Fig. 19 assumes that the peak outflow rate occurs at the moment when the pondage has all been drained. This does not appear likely. If Mr. Izzard's triangular hydrograph of outflow from ponds is correct, it bears out his conclusion that the flat topped hydrograph, when summated, would give a considerably higher rate of runoff early in the storm.

In adding outflow rates for ponded areas, it was felt unnecessary to develop the recession part of the ponded hydrograph, inasmuch as the peak value was all that was significant for design purposes. If it had been necessary, it would have been developed on the assumption that Mr. Izzard makes in Fig. 19. The

recession curves in Fig. 13 were sketched in with the area beneath them equal to the volume of pondage less infiltration during the recession side of the curve.

Fig. 20 indicates a variation in technique for the development of a net rainfall curve. This deserves consideration in evolving a less laborious methodology for handling the problem.

Mr. Hicks inquires concerning the hydraulic grade lines, as shown in Figs. 9 and 12. These were developed originally on the presumption that the pond would not have a free hydraulic relationship with the collecting pipe line—that is, an intervening short connection would prevent the pond from having a direct influence on the hydraulic gradient of the upstream pipe. Admittedly, the actual hydraulics involved in the pond is complex and suggests the need for some careful research to indicate the actual hydraulic conditions that would prevail under circumstances comparable to airport ponded drainage.

The writer is very appreciative of the considerable interest evidenced by the provocative discussions.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

ANALYSIS OF STEPPED-COLUMN MILL BENTS

Discussion

BY ERIK SOLLID, A. FLORIS, CHARLES W. DOHN, AND DANIEL S. LING

ERIK SOLLID,[17] ESQ.—The work that the author has done in connection with the design of stepped columns is time saving and valuable. Curves similar to the ones presented by the author are in use in the design office of the Bureau of Reclamation (USBR). They were prepared chiefly by L. W. Crandell and H. L. Neve.[18] In addition to curves similar to those shown by Mr. Ling, the USBR memorandum includes a set of curves giving the fixed-end moments for a concentrated load anywhere along the member having both ends fixed, or having the heavier end fixed and the smaller end hinged. It was found that by letting each curve represent a different ratio of

$$m = \sqrt[3]{\frac{I}{I_o}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (29)$$

—instead of the ratio x as used by the author, more orderly curves were obtained—that is, curves that did not have the tendency to intersect or twist around one another as is especially apparent in Figs. 6, 8, and 9.

It is dangerous for a designer to accept any design curves or diagrams, however, without the satisfaction that they are correct. Therefore, the writer wishes to call attention to a simple method for spot checking the diagrams without resorting to the more laborious methods of moment area or column analogy. As seen from Fig. 9, the signs of the fixed-end moments for a moment applied at the change in section varies both with the $\frac{I}{I_o}$-ratio and the value of x, and it is impossible to determine the signs for the fixed-end moments by inspection. In the spot check discussed herein, however, it is a simple matter

NOTE.—This paper by Daniel S. Ling was published in October, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1947, by Ralph E. Spaulding; and May, 1948, by E. I. Fiesenheiser, Walter J. Gray, L. J. Mensch, and Arthur R. Graves.

[17] Engr., Bureau of Reclamation, U. S. Dept. of Interior, Denver, Colo.

[18] "Concrete Building Design Data," by L. W. Crandell, George Evans, Erik Sollid, H. L. Neve, and Samuel Judd under the direction of L. N. McClellan, *Technical Memorandum No. 629*, U. S. Bureau of Reclamation, Denver, Colo., November, 1943.

to determine the direction of the end moments. The method consists of treating the member as a continuous beam with one redundant support.[19] Instead of using the author's sign convention, the writer prefers the following one: If the moment at the end of a member tends to cause clockwise rotation of the adjacent joint into which the member frames, that moment will be called positive (+); conversely, a moment tending to cause counterclockwise rotation is negative (−).

In the following examples a member (as shown in Fig. 24) will be used. This member is similar to the column ab in Fig. 4.[19a]

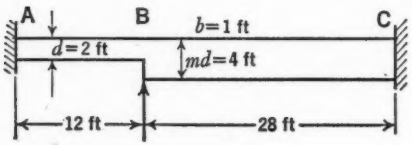

FIG. 24

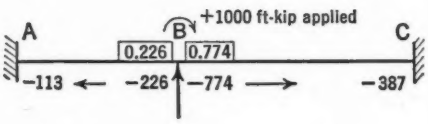

FIG. 25

Tentatively the beam will be supported at point B (Fig. 24), thus defining a continuous beam of two uniform sections. The frame properties of these beams are then as follows: $I_{AB} = \frac{b\,d^3}{12} = \frac{1 \times 2^3}{12} = 0.666$ and $I_{BC} = \frac{1 \times 4^3}{12} = 5.333$.

Case I.—Assuming points A, B, and C fixed, the stiffness ratio equals $K_{AB} = \frac{I}{L} = \frac{0.666}{12} = 0.0555$ and $K_{BC} = \frac{5.333}{28} = 0.19$. The distribution factors around joint B are $r_{BA} = \frac{0.0555}{0.0555 + 0.19} = 0.226$ and $r_{BC} = \frac{0.19}{0.2455} = 0.774$.

Case II.—Assuming point A hinged and points B and C fixed, $K_{BA} = 0.0555 \times 0.75 = 0.0416$ and $K_{BC} = 0.19$, as for Case I. The distribution factors are $r_{BA} = \frac{0.0416}{0.0416 + 0.19} = 0.18$ and $r_{BC} = \frac{0.19}{0.2316} = 0.82$.

Example 1.—Determine the fixed-end moments due to an applied moment at point B (Fig. 24), the change in section.

In Fig. 25 only one distribution around joint B is necessary to find the moments created by an applied load of 1,000 ft-kips at point B. Distribution factors are shown in rectangular boxes and moment carry-overs are indicated by arrows. The reaction on the support at point B will be upward and equal to $-(226 + 113)\frac{1}{12} + (774 + 387)\frac{1}{28} = 13.2$ kips. The true fixed-end moments will now be the moments shown at ends A and C in Fig. 25, plus the moments caused by an upward force of 13.2 kips at the change in section. To find the moments resulting from this force, hold joint B against rotation and give it an upward deflection of $\Delta = 10{,}000\,z$. The quantity 10,000 is chosen arbitrarily and z is the correction factor. The fixed-end moments in spans AB

[19] "Continuous Frames of Reinforced Concrete," by Hardy Cross and N. D. Morgan, John Wiley & Sons, Inc., New York, N. Y., 1932.

[19a] Correction for *Transactions:* In October, 1947, *Proceedings*, on page 1276, rotate Fig. 4 counterclockwise 90°.

and BC resulting from this deflection are

$$M_{AB} = M_{BA} = \frac{6\,E\,I}{L^2} = \frac{6 \times 0.666}{12^2}\,10.000\,z\,E = -\,278\,z\,E$$

and

$$M_{BC} = M_{CB} = \frac{6 \times 5.333}{28^2}\,10.000\,z\,E = +\,408\,z\,E$$

Next, let joint B rotate and distribute the moments as shown in Fig. 26.

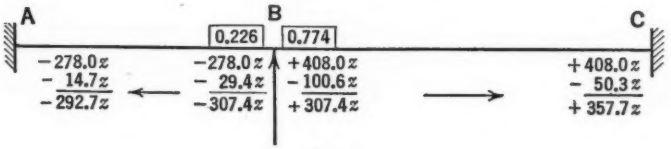

FIG. 26

The reaction at point B due to these moments is downward and equal to $73.8\,z$. Setting this reaction equal to the reaction of 13.2 kips, caused by the moments in Fig. 25, gives $z = \frac{13.2}{73.8} = 0.179$. The correct fixed-end moments caused by a moment of 1,000 ft-kips, applied as shown in Fig. 25, and without any support at point B, are now

$$M_{AC} = -\,113 - 292.7 \times 0.179 = -\,165.4 \text{ ft-kips}$$

and

$$M_{CA} = -\,387 + 357.7 \times 0.179 = -\,323.0 \text{ ft-kips}$$

For an applied moment of − 120 ft-kips at the change in section, the fixed-end moments are

$$M_{AC} = -\,165.4 \times \frac{-120}{1{,}000} = +\,19.9 \text{ ft-kips}$$

and

$$M_{CA} = -\,323.0 \times \frac{-120}{1{,}000} = +\,38.8 \text{ ft-kips}$$

which agree with the results given by the author in the paragraphs containing Eqs. 8.

Example 2.—Determine the fixed-end moment generated by a concentrated transverse load of 8 kips at the change in section, as shown in Fig. 4. In Fig. 26 the moments have already been obtained for an arbitrary deflection at point B. It is recalled that these moments produced a downward reaction of $73.8\,z$. The moment resulting from the 8-kip load is then simply $\frac{8}{73.8} = 0.108$ times the moments in Fig. 26, or

$$M_{AC} = -\,292.8 \times 0.108 = -\,31.6 \text{ ft-kips}$$

and

$$M_{CA} = +\,357.7 \times 0.108 = +\,38.6 \text{ ft-kips}$$

This compares to 31.8 and 38.7, respectively, as given by the author—a very good agreement. Finding carry-over factors and stiffness factors is equally simple. By definition, the carry-over factor is the ratio between the resulting moment at the fixed end of a beam to the moment applied at the other end of the beam. The stiffness factor of a member is equal to the moment required to turn the free end of the member through a unit rotation when the other end is held fixed or free. These are not the general definitions of the stiffness and carry-over factors, but they suffice for the present purpose.

Example 3.—Find the carry-over factor from end A to end C, in Fig. 24. This is done merely by applying a unit moment at end A, considering it as hinged. After distributing around joint B, the moments are as shown in Fig. 27.

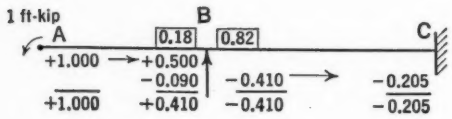

FIG. 27

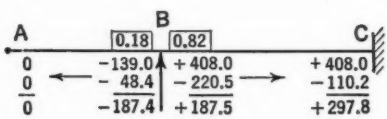

FIG. 28

The reaction at point B is upward and equal to $\frac{1 + 0.41}{12} + \frac{0.41 + 0.205}{28}$ $= 0.14$. In this case, with joint A hinged, the moments generated by an arbitrary upward deflection at point B of $\Delta = 10.000\,z$, after joint B has been allowed to rotate, are as shown in Fig. 28. Moments are in terms of z, the correction factor.

The reaction at point B is downward and equal to $\frac{187.4\,z}{12} + \frac{187.5 + 297.8\,z}{28}$ $= 32.9\,z$. Equating this reaction to the reaction of 0.14, from Fig. 27, gives $z = \frac{0.14}{32.9} = 0.00426$; and the moment at end C is

$$M_{CA} = -0.205 + 297.8 \times 0.00426 = +1.055$$

Since a unit moment was applied at end A, the moment at end C is equal to the carry-over factor. The author gives 1.06—which is also a very good agreement.

The stiffness factor is found in a similar way, but in this case, end A is held rigid while the distribution and the deflection correction are made. The final moment at end A will now be equal to the ratio between the stiffness of the beam of constant section AB, to the stepped beam AC. A more direct way would be to apply the stiffness factor of the constant section AB as a moment at end A. Then after completing the foregoing distribution and deflection correction, the final moment at end A would be the stiffness factor for the stepped beam AC. The correctness of this follows directly from the definition.

It is believed that the foregoing discussion will help a designer in gaining confidence in the design curves, and especially will help in checking as to the direction of the end moments in a beam with a moment applied somewhere in the span—usually a difficult and confusing problem.

A. FLORIS,[20] ESQ.—At great length well-known expressions are derived by well-known methods and applied to the analysis of stepped-column mill bents in this paper. In the mind of a critical reader doubts will arise as to the scope, usefulness, and value of such efforts. Since the introduction of relaxation methods the importance of tables and charts in the analysis of rigid frames and continuous beams with variable moments of inertia has been unduly exaggerated. The integrations involved in the evaluation of various constants can be replaced by summations, and results can be obtained in a comparatively short time. The great advantage of the summation process lies in the fact that it is applicable to all conceivable cases; its use is not restricted to the number or completeness of available tables and diagrams.

Although the application of the moment-distribution method to framed structures is not difficult, the author prefers to replace the framed supports by stepped, solid columns. Thus, at the cost of accuracy an approximation is introduced which could be avoided. Inasmuch as the author uses only solid columns, the present discussion will be confined to this type of structure.

By using one of the general methods of mechanics, all the necessary expressions of the relaxation methods can be derived, quite simply, as follows: Adapting the notation of the paper, let

$$C_2 = \lambda = \frac{1}{L^2}\int_o^L \frac{(L-x)\,x}{I_x}\,dx \qquad (30a)$$

$$C_1 = \zeta = \frac{1}{L^2}\int_o^L \frac{x^2}{I_x}\,dx \qquad (30b)$$

and

$$C_3 = \rho = \frac{1}{L^2}\int_o^L \frac{(L-x)^2}{I_x}\,dx \qquad (30c)$$

Then

$$\phi = C_1 C_3 - C^2{}_2 \qquad (31)$$

For the joint rotations, the stiffness factors are as follows:
At the far end—

$$\Lambda = \frac{C_2}{\phi} \qquad (32a)$$

and at the near end—

$$Z = \frac{C_1}{\phi}; \qquad P = \frac{C_3}{\phi} \qquad (32b)$$

For sidesway or bar rotations, the stiffness factors for the far and near ends are, respectively,

$$Y = \frac{C_1 + C_2}{\phi}; \qquad \Omega = \frac{C_3 + C_2}{\phi} \qquad (33)$$

Carry-over factors for joint a and joint b (Fig. 29) are C_2/C_1 and C_2/C_3, respectively.

[20] Los Angeles, Calif.

In addition, the fixed-end moments are expressed by

$$M_{f,A} = \frac{1}{\phi L}(F_a C_1 - F_b C_2) \ldots\ldots\ldots\ldots\ldots\ldots (34a)$$

and

$$M_{f,B} = \frac{1}{\phi L}(F_b C_3 - F_a C_2) \ldots\ldots\ldots\ldots\ldots\ldots (34b)$$

in which

$$F_b = \int_o^L \frac{M_{ox}\, x}{I_x}\, dx \ldots\ldots\ldots\ldots\ldots\ldots (35a)$$

and

$$F_a = \int_o^L \frac{M_{ox}\,(L - x)}{I_x}\, dx \ldots\ldots\ldots\ldots\ldots\ldots (35b)$$

In Eqs. 35, M_{ox} is the bending moment for the given loading of the beam on two supports.

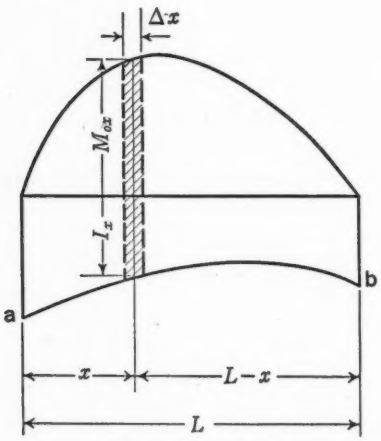

FIG. 29.—STRIP SUMMATION

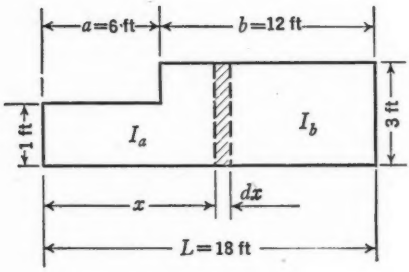

FIG. 30.—STEPPED BAR

In all the foregoing equations C_3 will be equal to C_1 if the beams or columns are symmetrical. The bending-moment diagram produced by an external load is shown in Fig. 29. Dividing this area into a convenient number of equal strips Δx, and replacing the integrals in Eqs. 30 and Eqs. 35 by sums, all the necessary quantities can be calculated. The same is true for unloaded beams and columns, in which case $M_{ox} = 0$. Eqs. 30 and Eqs. 35 are applicable to all cases including stepped columns, the simplest of all.

For comparison the constants of a solid column, 1 ft thick and 18 ft long (Fig. 30), are given in Table 7. In case 2 the column was divided into nine equal strips with $\Delta x = 2$ ft, and by means of Fig. 29, the column factors determined. Case 3 gives the basic expressions C_2, C_1, and C_3 taken from Figs. 6 to 9, and the remaining quantities were calculated. The great discrepancy between values for case 1 and those of the author (case 3) raises a

fair question as to the reliability of these charts. In order to check the accuracy of the summation method, the integrals of the basic quantities have been evaluated. The results are

$$C_2 = \frac{1}{6\,L^2}\left(\frac{\delta}{I_a} + \frac{L^3 - \delta}{I_b}\right) \ldots\ldots\ldots\ldots\ldots\ldots (36a)$$

$$C_1 = \frac{1}{3\,L^2}\left(\frac{a^3}{I_a} + \frac{L^3 - a^3}{I_b}\right) \ldots\ldots\ldots\ldots\ldots\ldots (36b)$$

and

$$C_3 = \frac{1}{3\,L^2}\left(\frac{\gamma}{I_a} + \frac{L^3 - \gamma}{I_b}\right) \ldots\ldots\ldots\ldots\ldots\ldots (36c)$$

TABLE 7.—COMPARISON OF RELAXATION FACTORS

Case	C_2	C_1	C_3	ϕ	Λ	Z	P	Y	Ω	$\frac{C_2}{C_1}$	$\frac{C_2}{C_3}$
1.....	10.30	5.04	51.40	153	0.0674	0.0329	0.336	0.100	0.403	2.05	0.205
2.....	10.44	5.17	51.59	158	0.0662	0.0328	0.327	0.099	0.392	2.02	0.203
3.....	13.50	6.70	70.00	287	0.0472	0.0234	0.244	0.070	0.291	2.02	0.193

In Eqs. 36,

$$\delta = a^2\,(3\,L - 2\,a) \ldots\ldots\ldots\ldots\ldots\ldots (37a)$$

and

$$\gamma = a\,(3\,b\,L + a^2) \ldots\ldots\ldots\ldots\ldots\ldots (37b)$$

For a bar of uniform section—that is, for $a = b = L$ and $I_a = I_b = I$, the foregoing expressions take the well-known form:

$$C_2 = \frac{1}{6\,I}; \qquad C_1 = C_3 = \frac{1}{3\,I} \ldots\ldots\ldots\ldots\ldots\ldots (38)$$

The values for case 1, Table 7, have been found by use of the foregoing expressions.

CHARLES W. DOHN,[21] JUN. ASCE.—The curves of elastic constants and moments for stepped-column mill bents, as presented in this paper, constitute a worthy addition to design knowledge. It is well to note attempts to apply rational methods to problems which, heretofore, were solved by making many broad assumptions. When the final sections chosen are no different from those obtained by prior methods, the more rational analysis serves to justify the many broad assumptions of the prior methods.

The author limits the value of his elastic constants and moments in the designs presented by making assumption *c* (under the heading, "II. General Conditions and Design Assumptions"), which assumes no end rotation of the truss. The statement is made that, unless the truss is subject to considerable deflection, this is a safe assumption. If the span length is rather long, as in the usual wide mill building, under vertical load the truss will exert considerable

[21] Engr., New York, N. Y.

moment on the small upper stepped-column section, and this plus other loading conditions has been found critical in previous designs with rigid top connections

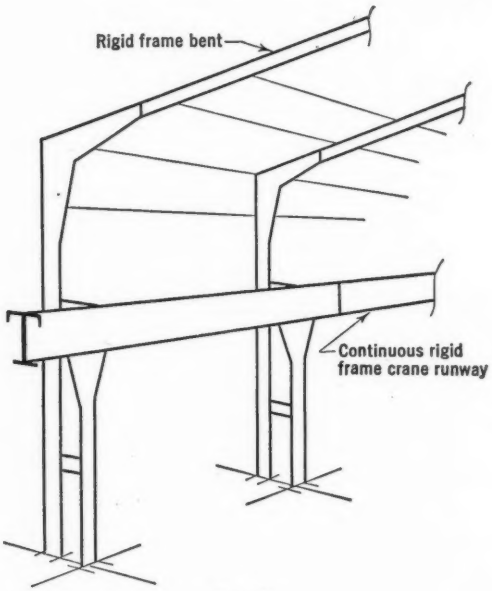

FIG. 31

of which the discusser has knowledge. The position of the end diagonal, in the truss shown in Fig. 4, would cause excessive stress in the lower chord of the end

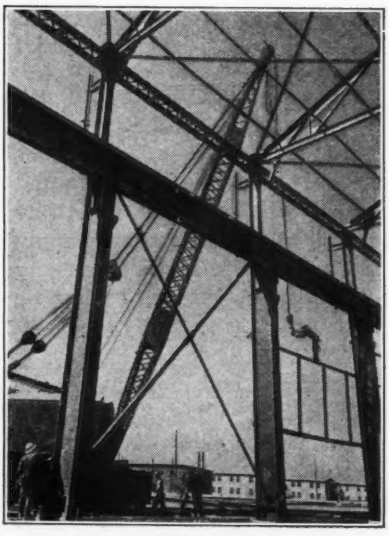

FIG. 32

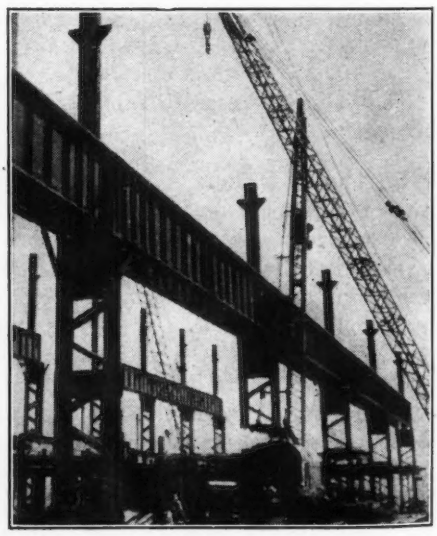

FIG. 33

panel. If the slope is reversed, thus bringing the end diagonal into the lower chord point at the column, a better stress condition prevails for a rigid joint of this nature. This is especially true for long spans. With further reference to Fig. 4, the author makes the statement:

> "The upper load of 32 kips will also produce some bending, particularly in the upper section; however, this will not be especially severe, since it is near the center line of the lower section."

True, it will not be a severe loading for the lower section; however, it is an extremely severe loading for the upper section when considered in conjunction with moments induced by other loading conditions. The upper step is limited by space requirements in its ability to resist bending. By making the neutral axis of the diagonal and the chord intersect at the neutral axis of the upper column section, the eccentricity of the end reaction may be eliminated, and moment on the upper column section may be reduced.

Example 1 would be more valuable if the complete analysis for all the eccentric loads shown in Fig. 4 were included. The 120-kip eccentric load will cause a translation of the column at the point of application of the couple—hence, the writer is not convinced that the method of superposition applies to Fig. 3(*e*). Inasmuch as bending is combined with axial compression, the deflection changes the action of the axial forces, and the latter produce not only axial compression, but also some additional bending. Small deflections may produce considerable changes in the action of forces.

The space requirements for clearance at the ends of the bridge crane and the difficulty of determining maximum moment conditions for a stepped column as part of a continuous and rigid structure subjected to various independent loads contribute to make the independent crane column economical of design, material, and construction. The diagrammatic sketch (Fig. 31) depicts a mill building with a continuous roof bent connected to a continuous rigid frame crane runway. The connections between the bent and the runway do not transmit vertical or bending forces, but may be utilized to absorb lateral shocks from the crane. Such a design is rigid, positive, economical, and more easily fabricated and erected than the stepped-column mill bent design. In the design of a rigid frame crane runway, such as indicated in Fig. 31, the method presented by R. C. Brumfield,[22] M. ASCE, would be applicable. The arrangement shown in Fig. 31 is, furthermore, unique, as it requires less longitudinal and lateral bracing than a stepped column. Aside from economy of material, this is an advantage at the working level. Argument might be presented that the stepped-column mill bent may also be made rigid with the crane girder. When the stepped column is made rigid with the girder and the roof, the resulting maximum stress conditions are not positively determinable without expenditure of excessive effort, and it is doubtful if such a design can match in economy one which is proportioned for the independent load conditions rather than an unfavorable combination of these dissimilar loading conditions. The independent column is not necessarily much less compact; it can be used

[22] "Moving Loads on Restrained Beams and Frames," by R. C. Brumfield, *Transactions*, ASCE, Vol. 111, 1946, p. 805.

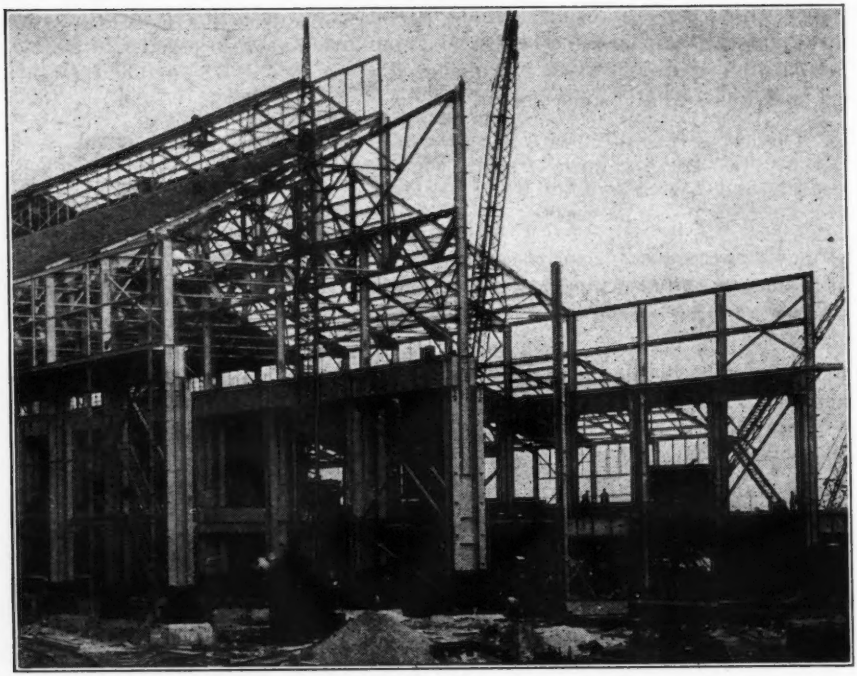

FIG. 34

FIG. 35

economically with wider and variable width bays; and it is more economical of crane girder and bracing material than the stepped-column mill bent when used in rigid frame designs. The independence of the building and the crane runway may increase the value of both. Either the building or the runway can be removed or altered independently to accommodate use, and the building is adaptable to the addition of multiple aisles. It would be debatable which type of column is best in resisting seismic forces. The independent system can be made as stiff or as flexible as requirements dictate; the stepped column is somewhat constrained in dimension and ill proportioned to resist lateral forces as part of a rigid frame.

The mill buildings shown in Figs. 32, 33, 34, and 35 were erected several years ago and represented good practice then. At present, and particularly in the future, continuous frames and rigid connections will vie more for better practice where good foundation conditions are available. The illustrations are presented in the hope of forming a more complete picture of typical engineering problems in selection and analysis of stepped-column designs wherein the author's elastic constants may be applied.

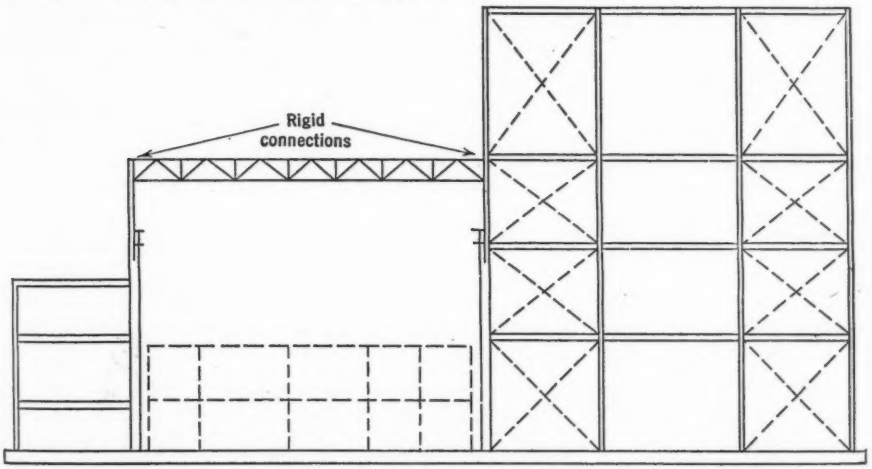

FIG. 36.—POWER STATION BENT

Fig. 32 shows a typical stepped column in a mill building. Note the longitudinal and lateral bracing and the rigid roof connections, not completed at the time the photograph was taken. The girders are not continuous and the bays are not very wide. Brackets are provided for knee braces to transmit longitudinal forces from the crane girder to the column, and thence to the bracing. These knee braces had not been added at the time the photograph was taken. The inside face of the column is a very deep channel section riveted to the lower column section. Brackets and a heavy billet are provided for the crane girder seat.

Fig. 33 illustrates another version of the stepped column wherein two stub columns support two crane runways and a roof column. Note that the bays are wide, and that the crane girders are deep, noncontinuous, and nonrigid.

Fig. 34 shows a heavier service, mill building group. Note the maze of bracing and the massiveness of the columns, as well as the deep crane girders, that result with simple spans and relatively nonrigid frames. (The high peak roof truss is not well adapted to use as a rigid frame member.) A stairway has been located so as to utilize part of the "dead" space between the columns. The mill building columns in Figs. 32, 33, and 34 present opportunity in their design for the elastic constants presented by the author.

Fig. 35 illustrates a multiple-aisle mill building with varying bay sizes, and with crane runways at different levels. The independent crane column was used for this difficult framing problem; however, the crane runway spans are neither continuous nor rigid, and the high peaked roof truss is not properly part of a proportioned rigid frame.

Mr. Ling's elastic constants for a stepped column would be useful in the design of a power station bent such as shown in Fig. 36. In this design the roof truss is rigidly connected to the upper crane column in the turbine room. The boiler house is braced for lateral forces. A study of moment connections was made for the boiler house but they were not used because they were not required as much as for, say, an office or apartment building. The bracing in the boiler house thus absorbs the lateral and sway forces from the crane-girder bent over the turbine room.

DANIEL S. LING,[23] M. ASCE.—The various discussions of this paper are greatly appreciated and the writer sincerely hopes that the original paper, clarified and enlarged on by the discussions, will be of practical use to the structural engineer.

Mr. Spaulding's objections are entirely valid. It was realized when the curves were originally developed that actually a gradual, rather than an abrupt, transition occurs at the column step. For most design problems, the error is not believed to be large, but if the designer prefers he can readily use the modified "x L," suggested by Mr. Spaulding.

Referring to the point of application of the side thrust from the crane, again, the writer must agree with Mr. Spaulding. This factor was considered when the curves were prepared but no ready solution was apparent without the complication of another variable, as the point of application of load in relation to the column step would vary with each size of crane girder. It was felt, however, that because of the stiffening effect of the usual crane girder connection the error involved would not be too large. Mr. Gray has suggested that fixed-end moment curves for other points of application would be desirable and Mr. Sollid has stated that such curves are available in a publication of the United States Bureau of Reclamation.

Mr. Gray's statement (2) is not clear. Distribution factors around any joint are simply the ratio of (C K)-values of the various members at the joint (see Table 1); and the use of curves would seem neither possible for the great variety of different conditions, nor of any practical benefit. Curves such as those suggested in statement (3) would, no doubt, have some use but would involve many series because of the variation not only in the point of applica-

[23] Cons. Engr., Detroit, Mich.

tion, but also in the terms x and I/I_o. The need for curves for carry-over factors is a matter of personal preference. Since the moments from translation derive from $(C_1 + C_2)\frac{E\,\Delta K}{L}$, the writer feels that a C_2-curve is preferable to a carry-over factor curve and has, in fact, often been forced to determine C_2-values from the carry-over curves in the series on haunched members published by the Portland Cement Association.

As to the statement that the design is not complete, the writer has noted that a complete design was not shown, but simply the determination of moments from bent action. The completion from that point should be no problem to the experienced designer and it was felt would add no worthwhile addition to the paper.

Mr. Mensch refers to the publications presenting formulas for bent analysis. Often, such bents will fit a case under consideration, but all too frequently the building to be designed does not seem to fit any of the cases and stepped column conditions are not included. Frankly, the writer has always been hesitant to use the sometimes lengthy formula because of great possibility of error due to misinterpretation, involved calculations, and hesitancy to accept a formula of whose accuracy he is not personally certain and which would take much time to check. The latter could, of course, be stated of the curves in Figs. 6 to 9, but a few spot checks are not too difficult.

Mr. Mensch's statement that the writer's moments at the foot of the columns are in error is not explained; certainly, it is at variance with Mr. Sollid's concurrence with the writer. In the examples given in this paper, the writer essentially assumed infinite stiffness of the roof truss. Although this assumption is, admittedly, apt to be considerably in error in many cases, it is readily possible for any assumed K-value of the truss to rotate around a "joint" midway between points a and a′, making use of the curves for determining C-values and fixed-end moments. The writer questions whether the usual roof truss connection to the column is adequate to develop the full stiffness of the truss.

In the time available the writer has not had the opportunity to study fully Mr. Mensch's method of solution. It seems applicable only to symmetrical bents and the writer does feel the greatest merit of his suggested solutions is for use with unsymmetrical bents.

Professor Fiesenheiser suggests a method of temporarily assuming a support at the column step. This suggestion is entirely feasible; and, if no curves for stepped columns were available, would be much easier than construction of the various $\frac{M}{E\,I}$-curves needed for a stepped section. With the use of the curves presented in the paper it would seem that Professor Fiesenheiser's method is somewhat longer, and especially so when bent effect is considered. It does, however, present another useful possibility to the designer when special conditions arise, and could adequately serve to handle the case of the applied loads or moments at points other than the column step. After moments for the applied forces were determined, the effects of bent translation would be found by the method presented in the paper.

Mr. Sollid presents the same general method as that given by Professor Fiesenheiser and suggests it as a spot check for the curves. His statement indicates that *Technical Memorandum No. 629*[18] should be a desirable addition to the reference library.

It is agreed, as Mr. Graves states, that the curves can be used for any type of problem for stepped members other than columns. The writer has knowledge, however, of unpublished curves that are more applicable to stepped beams or slabs. With reference to the moment at the top of the column, the designer should, of course, consider the worst possible combination of loads. Perhaps, Mr. Graves' suggestion of not less than 50% of the fixed-end moment has merit.

Mr. Graves' statement is correct; the curves in Figs. 6 to 9, inclusive, are included in "Statically Indeterminate Structures."[2] Permission had been granted to L. C. Maugh, M. ASCE, to include the curves in his book and because of wartime difficulties and restrictions the book was published prior to the publication of this paper.

If, by "solid" columns, Mr. Floris means a full, rectangular section, he appears to have misinterpreted this paper. The curves are applicable to any section for which the moments of inertia can be determined and the examples in the paper show standard rolled beam sections. The inference regarding integration is not understood. Actually, the writer's curves were prepared by summations from the $\frac{M}{E\,I}$-diagrams, essentially the same process as Mr. Floris' method.

Mr. Floris' C-factors are not the same as the writer's and, therefore, are not comparable. The difference is readily noted in Eq. 38, in which all the C-values include I; whereas, in the slope-deflection equations for uniform section, $C_1 = C_3 = 4$ and $C_2 = 2$. In view of Mr. Floris' statement regarding accuracy, the point should be made that the writer determined, by the conjugate beam method, the slope-deflection coefficient for the case of Fig. 30 in which x (Fig. 1) $= 0.33$ and $\frac{I}{I_o} = \frac{3^3}{1^3} = 27$. The values are: $C_1 = 6.93$, $C_2 = 13.6$, and $C^3 = 68.3$—which, considering the small scale of the curves, are in good agreement with the values determined (case 3, Table 7) by Mr. Floris from the curves.

For a correct comparison of Mr. Floris' factors with those of the writer, the term Λ is the writer's C_2 multiplied by K_o, $Z = C_1 K_o$, $P = C_3 K_o$, $Y = (C_1 + C_2) K_o$, and $\Omega = (C_3 + C_2) K_o$. For the column of Fig. 30, using foot units, $I_o = \frac{1}{12}$, $L = 18$, or $K_o = \frac{1}{12 \times 18} = \frac{1}{216}$. With this multiplying factor, case 3, Table 7, should read $\Lambda = 0.062$, $Z = 0.031$, $P = 0.0324$, $Y = 0.098$, and $\Omega = 0.386$; and, if the values obtained from the $\frac{M}{E\,I}$-curve were used, the values for case 4 would be: $C_2 = 13.67$, $C_1 = 6.93$, $C_3 = 68.3$, $K_o = \frac{1}{216}$, $\Lambda = 0.0633$, $Z = 0.0321$, $P = 0.316$, $Y = 0.096$, $\Omega = 0.380$,

[2] "Statically Indeterminate Structures," by L. C. Maugh, John Wiley & Sons, Inc., New York, N. Y., 1946.

$C_2/C_1 = 1.97$, and $C_2/C_3 = 0.200$. It is thus noted that Mr. Floris' "great discrepancy" does not exist. It would appear that Mr. Floris is simply presenting another, somewhat similar method of obtaining stiffness factors and fixed-end moments, which is as time consuming as the method used by the writer in preparing the curves presented in this paper.

Mr. Dohn's statements regarding design conditions will not be challenged. It is realized that one cannot generalize greatly with respect to design assumptions because what may not be too important in one case, may, indeed, be serious in another. In the main, the writer wished to present his curves for use where applicable.

The argument of stepped columns versus independent columns is, to a great extent, a matter of opinion; but the writer has felt that the objection to stepped columns simply on the basis of difficulty of analysis should not hold. Actually, in many designs using independent columns, the crane columns are so thoroughly connected to the building column with diaphragms or lacing that essentially a stepped column results. The use of knee braces on crane girders is, to the writer, most questionable. Too often such knee braces are used without any consideration being given to the bending produced in the column from the deflection of the crane girder, with consequent overstressing of the column.

The writer wishes to thank all the discussers of the paper and is pleased that several of them feel that the curves presented will be of use to the designer.

Corrections for *Transactions:* In October, 1947, "Proceedings," on page 1277, line 1, change "point b" to "point a"; Fig. 5(*a*), interchange the joint designations "a" and "b"; in Table 1 (in the title) change "Point b" to Point a" and (in the first column) change "bc" to "aa'"; and, on page 1284, in the second line of the last paragraph of Section IV, delete "by" and substitute "to problems in."

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

CLASSIFICATION AND IDENTIFICATION OF SOILS

Discussion

BY ARTHUR CASAGRANDE

ARTHUR CASAGRANDE,[22] M. ASCE.—The discussions have rounded out the contents of the paper by the addition of valuable material and of many helpful suggestions, for which the writer is grateful.

There is one misunderstanding noticeable in several of the discussions which is caused by the brevity of paper—namely, that the purpose of the Airfield Classification (AC) System is merely to classify the raw material of which the soil is composed, without regard to the engineering properties of the material in its undisturbed state. This interpretation is definitely not the writer's intent. All the data listed in Cols. 1 to 6, Table 4, must be considered as an integral part of the complete classification. Cols. 1, 2, 3, 4, and 6 classify the material in the "disturbed state"—that is, the raw material. In Col. 5 are listed characteristics which refer to the engineering properties of the material in the condition in which it will be used by the engineer. Since Table 4 is prepared specifically for the use of the AC system in airfield projects, the observations and tests listed in Col. 5 are selected for this purpose. When dealing with other types of engineering projects, Col. 5 will obviously require revision. No formal grouping has been proposed for the data which are to be included in Col. 5. However, it is conceivable that, for specific engineering projects, the observations and tests relating to the material in place could be subjected to further classification.

Several of the discussions have pointed out that in many cases a geologic classification of the soils should be included in the general classification, and that use should be made of local soil names. The writer agrees with these views and suggests that another column should be added in Table 4 with the heading, "Geologic Description and Local Name." In this connection it should be noted that there is also some danger in the use of local soil names because

NOTE.—This paper by Arthur Casagrande appeared in June, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1947, by Ralph E. Fadum; October, 1947, by James H. Stratton, and Donald J. Belcher; November, 1947, by J. A. Haine and J. W. Hilf, and Jacob Feld; January, 1948, by Kenneth S. Lane, George F. Sowers, René S. Pulido y Morales, Raymond F. Dawson, and D. F. Glynn; March, 1948, by L. F. Cooling, A. W. Skempton, and R. Glossop, Milton Vargas, Donald M. Burmister, and M. G. Spangler; and May, 1948, by E. W. Lane, and B. K. Hough.

[22] Prof. of Soil Mechanics and Foundation Eng., Graduate School of Eng., Harvard Univ., Cambridge, Mass.

in different parts of the United States they may refer to materials differing greatly in their engineering characteristics. For example, the term "hardpan" has led to numerous lawsuits in which the lawyers had no difficulty in finding in the literature definitions which suited their individual purposes. It seemed to the writer that the older the quotation the more weight it carried in the Court of Law.

Professor Fadum offers several practical suggestions for promoting agreement on soil terminology. The writer agrees with Professor Fadum that it would be more significant if the Atterberg limits were performed on material smaller than 0.1 mm in diameter, rather than on the fraction passing the No. 40 sieve. Since separation of the fraction smaller than 0.1 mm is cumbersome and time consuming, general acceptance of such a suggestion is not probable until a much faster procedure is developed for the separation of this fraction.

Colonel Stratton reviews the circumstances leading to the development of the AC system, and demonstrates in Fig. 7 how the system was combined with the design procedure for flexible pavements, which was developed under his direction by the Office of the Chief of Engineers, United States Army. Then he reviews the comments made in 1942 by various divisions and districts of the United States Engineer Department—that is, soon after the AC system was adopted as a tentative standard by the Corps of Engineers for military construction. More recently, a modification was developed by the Office of the Chief of Engineers for civil works purposes, based on the experience gained in the use of the AC system since its inception.

Professor Belcher presents a competent discussion on the advantages of the pedological classification. It is true that the thoroughness of exploration on which a pedological classification of a given locality is based results in information which is of value to the engineer, and that much time and effort can be saved by utilizing such available information. However, the writer does not believe that the pedological classification is suitable in general for engineering purposes.

Messrs. Haine and Hilf describe an adaptation of the AC system which is used by the Bureau of Reclamation for use with earth dam projects (59).[22a] They emphasize that proper field identification is the primary objective in soil surveys, and found it necessary to subdivide the GF and SF groups according to the type of fines. This step is logical, particularly in connection with the use of soils for the construction of rolled earth dams. In addition, Messrs. Haine and Hilf have rated the various groups of materials according to their usefulness in the design of earth dams—a helpful supplement when using this classification for earth dam projects.

Mr. Feld has brought a welcome diversion into this rather prosaic subject, by reviewing some of the history of soil classification.

Kenneth S. Lane first presents an analysis of the requirements which a classification system should fulfil, and then proposes a new system referred to as QPC (Quabbin, Providence, and Casagrande) classification, in which he combines the advantages of several systems by using a descriptive name, a grain-size

[22a] Numerals in parentheses, thus: (59), refer to corresponding items in the Bibliography (see Appendix of the paper), and at the end of discussion in this issue.

number, and the group symbol of the AC system. The writer is in full accord with Mr. Lane's discussion of typical soil names, with emphasis on the geology of the soils. The use of a grain-size number is a desirable supplement in classifying many coarse-grained soils of a great variety, such as are encountered in glacial deposits. In such instances, a man could be trained to estimate the grain-size class by visual inspection. However, when working in areas where coarse-grained soils are rather limited in extent, the necessary skill for a fair differentiation between thirty-five grain-size classes will usually not be available. If high accuracy in classifying the grain-size distribution is needed, one will have to depend on laboratory analyses. Because of Mr. Lane's extensive experience, his recommendations deserve serious consideration by those who wish to use a comprehensive method for classifying soils.

Professor Sowers points out the difficulty resulting from the fact that the term "plastic" is used by builders and drillers to designate the consistency of a soil in its natural state. He also emphasizes the importance of a detailed description of the properties of the undisturbed material and suggests the use of numerical scales to replace the uncertainty of descriptive adjectives. The writer believes that in visual and manual examination only a small number of grades can be distinguished, which are better described by generally understood adjectives. However, there is no question that a quantitative basis should be established for the purpose of standardizing the use of the adjectives.

Mr. Pulido y Morales expresses preference for the modified Public Roads (PR) Classification. He compares (Table 7) the group symbols for forty-four different soils according to the original and the modified PR classification, which to the writer demonstrates the confusion that must result if both classifications, which differ so greatly, make use of the same group symbols.

Because of space limitation the writer must forego a detailed discussion of the group index in the modified PR classification, and of the relationship between the liquid limit and compressibility, which Professor Dawson desires.

Mr. Glynn points out that in Australia a descriptive soil classification has been widely used as the basis of field identification. He suggests that, to prevent confusion, further subdivisions in the AC system should be limited to special applications. The writer is inclined to agree with this suggestion.

Many of the features which are insufficiently presented in the AC system are very well covered in the classification proposed by Messrs. Cooling, Skempton, and Glossop in Table 8. Although specifically adapted to the soil conditions in England, Table 8 may well serve as a general basis for establishing similar classifications in other parts of the world. In Fig. 11, the relationship between the liquid limit and the clay fraction (percentage smaller than 2 microns) is plotted, to illustrate that, for two clays having the same liquid limit, the clay with the more potent colloidal content has the smaller clay fraction. Whether such a relationship will yield information in addition to what can be derived from the plasticity chart, is questioned. Besides, difficulties arise when one also plots the results of materials with high liquid limit which lie well below the A-line, such as organic clays, or micaceous and diatomaceous soils.

Mr. Vargas suggests that the organic and inorganic silts may be grouped together into a major division. The writer questions the value of such a com-

bination, since organic silts and inorganic silts are really quite different types of soils.

Mr. Vargas raises the question as to whether the group symbols, which are abbreviations of English names, should in other languages be changed to the initials of the corresponding names. He answers his question by suggesting that the original symbols should also be retained in other languages. This would certainly prevent confusion. The writer admits that in devising the symbols he has not thought about the possibility that this classification might spread to other countries. The purpose of the group symbols was to give the user something that would not tax his memory. Thus, it would be logical to create new symbols in each language. On the other hand, if this classification should become widely used, it would be desirable to have only one set of symbols which are internationally accepted. There is already one symbol in these groups which is not derived from the English language, namely, the letter, M. Considering that the majority of important references in soil mechanics are, and probably in the future will be, in the English language, and that English is the only language used for the International Soil Mechanics Conferences, the adoption of the present symbols as an international standard would not be too great a hardship. It would mean that, in teaching the classification, the student would have to learn the English names of about one dozen soils. After he has mastered these words, the use of these symbols would be just as understandable to him as if the symbols referred to the names of these soils in his own language.

Professor Burmister proposes to broaden the AC system by a better definition of the principal soil components, and by establishing a common language in describing soils. His discussion contains many valuable suggestions, which merit attention in any future improvements of existing soil classifications.

Professor Spangler has presented a competent defense of the new PR system. By including sufficiently detailed information on this new system, he has contributed a necessary supplement to the paper. Professor Spangler expresses regrets that other organizations deemed it necessary to adopt an entirely new system, when the PR system would have been satisfactory. However, that the original PR system was not satisfactory was admitted tacitly by those who created the new PR system, after the AC system was already in existence. In fact, it seems to the writer that, except for the symbols, the new PR system is closer to the AC system than to the 1929 PR system. Therefore, the writer reiterates that it would have been less confusing and more straightforward if the new PR system had received a new and more logical set of symbols, instead of retaining the original symbols.

Professor Spangler misunderstood the writer's remark regarding the revisions of Table 4 to mean that the AC system has been revised three times since its inception in 1942. Some minor revisions were made in Cols. 12 to 15, Table 4, which are not an inherent part of the AC system.

If none of the existing methods for identifying and classifying soils for engineering purposes makes use of their mineralogical and chemical composition and surface area, the reason is simply that the very complex relationships between these characteristics and the engineering properties of soils have not

been established to any useful and practical extent. The writer, however, agrees with Professor Hough that more research along these lines is desirable.

E. W. Lane discusses a new grain-size classification recently proposed by the American Geophysical Union. In soil mechanics there has not developed, so far, any need for such detailed grain-size classifications, and the writer doubts that they will ever be needed.

The writer should like to point out that the soil classifying procedures actually used by the Bureau of Reclamation and by the Corps of Engineers, as well as by the Bureau of Yards and Docks, United States Navy (the latter according to written information received from L. A. Palmer), differ only in minor points. It would seem to the writer feasible and eminently desirable for these three large users of soil mechanics to initiate a cooperative effort, which should eventually lead to a uniform soil classification. If such an effort succeeds, it would constitute the most important step toward resolving order from the chaos of the past.

Bibliography.—

(59) **"Earth Materials Investigation Manual,"** Bureau of Reclamation, U. S. Dept. of the Interior, Denver, Colo., November, 1947.

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DISCUSSIONS

SEA LEVEL PLAN FOR PANAMA CANAL TO PROVIDE MAXIMUM SAFETY AND UNLIMITED CAPACITY

Discussion

BY HANS KRAMER, AND JAMES H. STRATTON

HANS KRAMER,[17] M. ASCE.—Although Mr Claybourn's authoritative paper and this discussion do not deal exclusively with current construction operations of immediate technical interest, they do presage construction activities of a nature and magnitude to challenge the capabilities of the construction industry. These potential and not far-distant activities deserve the most sober consideration of the Society from a professional viewpoint, and of the public at large as a matter of national concern.

Since the preparation of Mr. Claybourn's paper, an important bill, directly connected with his subject, has been enacted by the Seventy-Ninth Congress and approved on December 28, 1945, as Public Law No. 280. The significance of this bill warrants quotation of its main provisions:

> "* * * the Governor of the Panama Canal, under the supervision of the Secretary of War, is hereby authorized and directed to make a comprehensive review and study, with approximate estimates of costs, of the means for increasing the capacity and security of the Panama Canal to meet future needs of interoceanic commerce and national defense, including restudy of the construction of additional facilities for the Panama Canal authorized by the Act approved August 11, 1939 (53 Stat. 1409). He shall also make such study without drafting plans or sketches as he may deem desirable to permit him to determine whether a canal or canals at other locations, including consideration of any new means of transporting ships across land, may be more useful to meet the future needs of interoceanic commerce or national defense than can the present canal with improvements. He shall report thereon to the Congress, through the Secretary of War and the President, not later than December 31, 1947."

In orienting this discussion to the over-all review contemplated by Public Law No. 280 it seems appropriate, at the outset, to attempt to dispel at least

NOTE.—This paper by J. G. Claybourn was published in February, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1948, by Fred Lavis and Raphael G. Kazmann.

[17] Brig. Gen., U. S. Army (*Retired*) Cons. Engr., San Francisco, Calif.

one popular illusion created by recent news items in connection with the inability of the latest supercarriers to cross the Panama Canal because of insufficient width of the existing locks. Contrary to assertions or intimations, this simple well-known fact does not indicate that someone has been "asleep at the switch," or that there is lack of teamwork between the United States Navy and the Canal Zone authorities. The dimensional limitations of the Panama Canal are, and have been, a matter of common knowledge, but their existence has been no deterrent to the construction and operation of the *Queen Mary* and a few similar superliners. The prewar carriers, the *Saratoga* and *Lexington*, with their 105-ft beams, were always a tight fit in the 110-ft locks, but their transits through the canal were always accomplished successfully. The federal government was well on the way toward enlarging those limitations by a new and larger third set of locks (which were authorized in 1939), when construction was suspended in May, 1942. That suspension, notwithstanding the desirability of having safer and larger facilities for war use, was compelled by serious shortages in shipping, manpower, and critical materials; it was due to the sound judgment of the Navy and Army, and of their commander in chief that these vital resources, then of greater importance to the war effort, were applied to other fields.

The Navy's construction in the subsequent and more abundant years of supercarriers too large for the existing locks was programmed with open eyes and in full anticipation of their usefulness in the later phases of the war, despite their inability to transit the Panama Canal. It is obvious, however, that the Navy's adoption of these larger dimensions for its newer vessels dictates canal facilities of corresponding size as soon as it is reasonably practicable to provide them. Whatever form those new facilities may take, the bulk of the $75,000,000 (of the $277,000,000 authorized), which was expended mainly on excavation and appurtenant work prior to suspension of the Third Locks Project, will undoubtedly prove to have been well spent; this work will be directly useful in the ultimate development of the project.

The activities and traffic of the Panama Canal were necessarily censored during World War II, but the uninterrupted functioning of the canal was of inestimable significance in the global war operations of the allied nations. If, and when, the full story is revealed, the United States will be prouder than ever of the tremendous benefits derived from the Panama Canal. Although the traffic capacity of the canal usually has been rated on a per annum basis, peak traffic conditions over shorter periods of weeks (or even days), such as occurred during the redeployment period in mid-1945, will undoubtedly have to be considered in gaging future traffic and facilities.

Thirty years of experience in the operation and maintenance of the present canal are reflected in Mr. Claybourn's paper, together with the advances in engineering technique and in the development of engineering tools, such as geology and soil mechanics over the same period; these advancements have extended engineering knowledge to a stage where engineers are much better able to design and construct works, such as cuts, embankments, and foundations with greater confidence in their ultimate performance. The preceding statement does not imply that all the answers to the engineering problems are simple

and readily available. The brief discussion (under the heading, "Deep Dredging Plan") for converting the Panama Canal into a sea level waterway in virtually a single operation with the aid of "* * * enormous dredges of great power, capable of excavating to depths of 135 ft * * *," exemplifies the problems to be solved and the unprecedented methods to be considered. It does imply, however, that the profession is blessed with greater enlightenment, and should, therefore, be cursed by less darkness in the solution of the next series of engineering problems; and it also implies that apprehensions of serious slides in the Panama Canal caused by improper sloping and draining in critical zones need not haunt future efforts.

Opposed to the advances of the past generation in the construction phases of engineering must be ranged the even greater advances in the destructive phases of the physical and military sciences. The destructive potentialities of the atomic bomb stagger the imagination and threaten man's very existence. However, even conventional bombing with long-range aircraft or in the form of remote-controlled missiles has destructive power which dictates a re-examination of the protective installations engineered in 1939. The resultant problem and the concern for the vulnerability of the Panama Canal will challenge all available technical skill and judgment, as never before. The development of a logical, feasible solution will necessarily absorb the attention of the ablest engineers and scientists, and will require the collaboration of the best minds of the Army, the Navy, the atomic bomb group, and other governmental agencies that have a stake in the future of the Isthmian canal.

In the comprehensive restudy to determine the next phase in the development of the Panama Canal, which is envisaged in Public Law No. 280 appropriate reconsideration must be given to the Nicaragua route and to other alternative sites. Although these alternatives have been disposed of in previous studies and decisions, they obviously deserve review, not only because of changes in physical factors affecting construction, but also in the light of the new geopolitical situation. No major field surveys are contemplated or necessary with respect to these alternatives, but an adequate reanalysis of factual data which are essential for purposes of comprehensive review is definitely in order.

In the restudy under Public Law No. 280 some popular fallacies must be dispelled again—for example, the misconception that the Nicaragua route is feasible for a sea level canal. Neither the Nicaragua route, nor any other alternative route has ever been considered practicable for anything but a lock canal; the Nicaragua plan involves a summit lake 110 ft above sea level against the 85-ft summit level of Gatun Lake at Panama. Again, it remains to be proved that the present Panama route was, and is, the only one engineeringly and economically feasible for construction of a sea level canal. Likewise, the oft-quoted adage, "It is unwise to carry all your eggs in one basket," which serves as a popular argument for the construction of another Isthmian canal at a location other than Panama, will have to be re-examined. However, the 1927 analysis of the "eggs-in-one-basket" argument together with the conclusion (stated previously by the

writer[11]) that "The Isthmian canal situation is not a question of carrying eggs, but of protecting baskets," is still pertinent and tenable today.

In conclusion the writer wishes to compliment Mr. Claybourn (and to congratulate the Society on its choice of subject and author) on his timely, forceful, and competent presentation of the problems involved in "Streamlining the Panama Canal for Maximum Safety and Unlimited Capacity" (see "Synopsis"). The writer has not attempted here to analyze the author's reasoning, nor to debate his conclusions, because he is confident that Mr. Claybourn's work will prove a valuable contribution and will receive thorough attention in the over-all review of the problem to be undertaken pursuant to Public Law No. 280. It needs to be emphasized, however, that, whereas a sea level canal at Panama has been heretofore a second choice (and rightly so), changed conditions have placed the burden of proof on those who would not advocate conversion to sea level. The restudy of the Isthmian canal problem must necessarily be governed by considerations of national defense which, in turn, are engendered by the specter of war. The successful solution of that problem should constitute another noble achievement for the engineering profession, and the nation, in their dedication to the constructive works of peace.

JAMES H. STRATTON,[18] M. ASCE.—The author presents a straightforward, practicable plan for the conversion of the present Panama lock canal to a sea level canal. Mr. Claybourn's proposal for the conversion to sea level is based primarily on the needs of national security, but he points out that the results of the conversion will be to satisfy the growing needs of commerce and, at the same time, provide a waterway both safe and satisfactory from a navigation viewpoint. Excepting for a few features, such as the method of excavation and lowering of the summit lakes of the Canal, some details of flood control, and the method of providing regulation of the Pacific tides, the plan described by Mr. Claybourn does not differ materially from that recommended by the Governor of The Panama Canal, J. C. Mehaffey, in his report submitted to Congress in compliance with Public Law No. 280, Seventy-ninth Congress.[19]

Mr. Claybourn describes both the multiple stage (step down) and single stage (deep dredging) plans for lowering the canal to sea level, and concludes that a combination of these plans would probably be more satisfactory inasmuch as this would permit using conventional-type dredges with minor modifications, instead of requiring special dredges capable of working to depths in excess of 135 ft. More recent extensive investigations of the single stage lowering plan (in which Mr. Claybourn has acted as dredging consultant), under Public Law No. 280, have revealed that single stage lowering of the Canal, requiring the use of special dredges capable of excavating to depths of 145 ft, is entirely practicable. Single stage lowering would result in a savings of approximately $130,000,000.

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[11] "The Isthmian Canal Situation," by Hans Kramer, *Transactions*, ASCE, Vol. 94, 1930, p. 406.

[18] Col., U. S. Army; Supervising Engr., Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

[19] "The Future and the Panama Canal," by James H. Stratton, in "Panama Canal—The Sea-Level Project: A Symposium," *Proceedings*, ASCE, April, 1948, p. 444.

The plan proposed by the author for the control of floods differs from that recommended in the report under Public Law No. 280, principally in features of design which are the outgrowth of extensive studies of recent date. The control plan for the Chagres River recommended in Governor Mehaffey's report contemplates that the Madden Dam and reservoir will operate in tandem with a dam at Gamboa to effect the control and diversion of floods, whereas Mr. Claybourn proposes that Madden Dam be made inoperative after the completion of construction. Nevertheless, it appears that with the removal of the control works, as he proposes, Madden Dam will serve to retard floods. The plan in the report, it is believed, will give greater flexibility of control than that proposed by the author. Although outlets and a spillway leading from Gamboa Dam are proposed in both plans, they would be used only in emergencies in the plan recommended to Congress. Thus, flows from the Chagres River into the sea level canal itself would be an extreme rarity, rather than one of occasional requirement which might result in the plan offered by Mr. Claybourn.

The author has stated the case for tidal regulation extremely well, pointing out that essential decision is not one of needing such regulation, but one of providing a sea level canal to meet security needs. The tidal regulating plan recommended in the studies under Public Law No. 280 provides for both a tidal lock and a navigable pass, the latter of full channel width. The proposed navigable pass meets the criterion stated by Mr. Claybourn—that, if tidal locks are provided, they should be designed so that their cross-sectional area is at least equal to the cross-sectional area of the canal prism.

Extensive studies of waterways throughout the world and model investigations at the Unites States Navy David Taylor Model Basin in Carderock, Md., made in conjunction with the investigations under Public Law No. 280, led to the adoption of a somewhat larger channel and an improved alinement compared with that suggested by the author; and this, in part, accounts for the higher cost of the plan for the conversion of the Panama lock canal to a canal at sea level recommended by the governor over that proposed by the author.

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DISCUSSIONS

DETERMINATION OF POSITION AND AZIMUTH BY SIMPLE AND ACCURATE METHODS

Discussion

BY JOHN C. PENN, PAUL E. WYLIE, AND P. V. H. WEEMS

JOHN C. PENN,[3] M. ASCE.—From a superficial examination, it may appear that Professor Hickerson has presented new and simplified methods of finding latitude, time, and azimuth.

Actually, the methods are not new; and they are not so simplified as those presented by A. A. Ageton in 1942.[4] Like the author, Lieutenant Ageton introduces the idea of multiplying the log secants and log cosecants by 10^5; and the use of the resulting substitution factors A and B by Professor Hickerson is also suggestive of the forms devised by Lieutenant Ageton.

The tables of A and B were designed to enable instructors to speed the inculcation of simple navigation. Use of the tables avoided entirely the conceptions of logarithms and spherical trigonometry, and reduced the entire subject matter to filling in forms, and to simple addition and subtraction. It is, of course, hardly to be expected that anyone would keep a table of values of A and B handy for a surveying problem that occurs only occasionally.

There are simpler methods of representing the zenith-pole-object triangle than those given in Figs. 1, 2, and 3. Few students or engineers have the imagination or the ability to see objects in space. It is most difficult for an observer to draw pictures of things infinitely large, and to place himself outside this infinitely large celestial sphere, when he is actually at the center of the sphere looking out. However, a student can readily visualize the spot on the earth pierced by a line from the sun, star, or planet to the center of the earth. This spot will have a latitude equal to the declination of the celestial body; its longitude will equal the Greenwich hour angle. The motion of this point on the earth can be studied in detail. Right ascension, so difficult to explain, can even be avoided, as it is in modern navigation.

As a revival of some tricks in trigonometry this paper may have value. More emphasis on Greenwich hour angle, local hour angle, and corrections for

NOTE.—This paper by T. F. Hickerson was published in November, 1947, *Proceedings*.

[3] Prof., Civ. Eng., Illinois Inst. of Technology, Chicago, Ill.

[4] "Dead Reckoning Altitude and Azimuth Table," by A. A. Ageton, *Publication No. 211*, Hydrographic Office, U. S. Navy Dept., Washington, D. C., 1942, p. 18.

transit altitudes would indeed be appreciated by the engineer and surveyor. To appraise the full potential value of the ideas presented, however, the careful student will need references to all prior work supporting the broad scope of this subject.

PAUL E. WYLIE,[5] M. ASCE.—The section under the heading, "Determination of Latitude," in this interesting paper appears to be an adaptation of a method of solution of the PZS triangle, which is widely used in navigation, and which is published elsewhere as "HO 211."[4] Tables of A and B to half minutes also may be found in the same publication.

The navigator solves his triangle for the altitude which would exist at an assumed geographical position—that is, with knowledge of ϕ and t_Z. Professor Hickerson, on the other hand, knowing t_Z and h_o, solves the same triangle for ϕ, using the same methods.

It will be interesting to discover circumstances in which this method of latitude determination is of practical utility. To use it, knowledge of the longitude is required; but the cases in which longitude is known and latitude is unknown are rare indeed. On the contrary, cases in which the latitude is known and the longitude is unknown are common enough, and in these cases the PZS triangle is solved by methods involving altitude instead of hour angle. In Example 1 the author has selected a small value of t_Z so as to minimize the error involved through uncertainty in the value of the longitude. This is quite legitimate, of course, but one wonders why the simpler method of meridian altitude was not employed if the observation must be made so near to meridian transit.

As the writer has stated elsewhere,[6] the latitude at upper or lower transit of any astronomical body may be found by

$$\phi = 90° - H + D \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (33)$$

in which $H = h_o$ when the body lies south of the zenith; $H = 180° - h_o$ when the body lies north of the zenith; $D = \delta$ when the body lies in the upper branch of the meridian; and $D = 180° - |\delta|$ when the body lies below the elevated pole, D being given the algebraic sign of the declination. Eq. 33 is, of course, a variant of the familiar equation,

$$\phi = z + \delta \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (34)$$

redesigned to involve altitudes, with which the surveyor and the navigator are familiar, instead of zenith distances, which are unfamiliar, and also to include all cases in one expression, inasmuch as Eq. 34 is limited to upper branch observations.

Eqs. 7 and 9 are new, as far as the writer has observed. They extend the use of "HO 211"[4] to the determination of t_Z and Z, respectively. Presumably, Eq. 7 would be limited in use to stars of low declination. It is probable that

[5] Lecturer, Dept. of Astronomy, Univ. of California, Los Angeles, Calif.

[6] *Popular Astronomy*, March, 1945, p. 108.

Professor Hickerson does not approve the use of Eq. 7 when t_Z is near zero, or the use of Eq. 9 on close circumpolar stars. These are correct limitations, but the advantage of the exclusive use of secants is not clear.

The writer shares Professor Hickerson's preference for hour angle methods over altitude methods for determining azimuths of close circumpolar stars; and he believes that the common preference for altitude methods on these stars is a carry-over from the days when it was difficult to determine the civil time of observation accurately. Since the advent of frequent radio time signals, it is a simple matter, given a reliable watch that has been rated for a short period, to know the time at any moment to the nearest second. The precision of the time determination is thus about four times as great as the precision of the corresponding altitude determination, assuming the use of an engineer's transit in perfect adjustment. By making the assumption, for a reasonably short period, that the change in azimuth of a close circumpolar star is proportional to the time interval (in other words, that the star's path is a straight line), the benefits of repetition on a horizontal angle may be employed; the observations may be much speeded (since no altitudes are read); and a superior precision of azimuth determination may be attained. The error of the assumption involved is negligible for a short arc of a circumpolar star's diurnal circle; moreover, it is involved to a greater degree in the slower altitude method.

Solution of the triangle for azimuth, using the hour angle method, is accomplished by employing

$$\tan Z = \frac{\sin t_Z}{\tan \delta \cos \phi - \sin \phi \cos t_Z} \quad \ldots\ldots\ldots\ldots\ldots\ldots (35)$$

It should be noted that, for five-place accuracy, when this equation is applied to Polaris, δ must be known to 0.1 sec of arc. Tabulations of the declination of Polaris at daily intervals to 0.01 sec of arc are available.[7,8] More condensed ephemerides of Polaris are not suitable.

In well-mapped country, position may be found with all necessary precision for circumpolar hour angle observations by consulting a good map, such as one of the quadrangle sheets of the United States Geological Survey, or a chart of the United States Coast and Geodetic Survey. In poorly mapped regions, a program of observations will be required to secure the data necessary for a precise determination of azimuth. Such a program might be as follows:

1. Determine latitude by maximum altitude of a star of low declination.
2. Determine longitude by altitude of a star on or near the prime vertical (solving for t_Z).
3. Determine latitude by Polaris, or another circumpolar body, at culmination.
4. Determine azimuth by Polaris at elongation. If no elongation occurs during the dark hours, use another circumpolar star at elongation, or substitute an observation on Polaris at any time, using the hour angle method.

[7] "American Ephemeris and Nautical Almanac," U. S. Govt. Printing Office, Washington, D. C.
[8] "The Ephemeris," Bureau of Land Management, U. S. Dept. of the Interior, Washington, D. C.

5. Set up a true meridian on the ground; redetermine longitude by observations of times of meridian passage of a series of equatorial stars.

6. If necessary, recompute step 4, using the revised longitude. This is in the nature of a check, and, since a small error in the longitude will have little effect on the azimuth of a close circumpolar body, the change indicated will usually be negligible. Consequently, steps 5 and 6 may be omitted, except in precise work.

Many engineers have become familiar with modern methods of navigation through wartime training. These persons may substitute for the foregoing program, a determination of position by the line-of-position methods used at sea, using a number of well-spaced stars, measuring the altitude of each (preferably with a sextant and artificial horizon) and noting the time of each observation. If "HO 211" is used for the computation of these observations, t_Z, as has been shown,[9] should not be near 90° for any star used.

It may be shown[10] that

$$dZ = \tan Z \cot t_Z \, dt \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (36)$$

which shows that an error in the time will have minimum effect on the azimuth when t_Z is near 90°, and that the most precise value of the azimuth will be found when Z is near 0° or 180°. In other words, the most accurate determination of azimuth is found when a close circumpolar star (Polaris) is observed —particularly when the observation of such a star is made at or near elongation. Those surveyors who use astronomical methods infrequently will no doubt continue to rise at some dark, frigid hour to observe Polaris at elongation, rather than troubling to learn other methods which, if understood, might permit a more conveniently timed observation. It may be a satisfaction to them to be reassured that the simple method with which they are familiar remains the most easily computed, the most accurate, and thus, the best.

P. V. H. WEEMS,[11] ESQ.—Dealing in terms of the civil engineer, and presenting convenient formulas for working accurately latitude, longitude, and azimuth, this paper should prove a valuable aid to practical surveyors and engineers. As explained by Professor Hickerson, the methods given are more accurate than are required by the marine navigator and for that reason are longer than those ordinarily used at sea. The author failed to mention his book,[12] which in a hundred pages gives not only a description of the methods, but also the actual tables necessary for accomplishing almost any survey or navigation problem.

The methods and principles involved are obviously correct and the symbols used are especially selected for the civil engineer. It is unfortunate that mariners use different designations for different parts of a triangle. This, apparently, is an excellent field for a national standards committee.

[9] "The Accuracy of Ageton's Method in Celestial Navigation," by Samuel Herrick, *Publications of the Astronomical Society of the Pacific*, August, 1944, pp. 149–155.

[10] "Textbook of Practical Astronomy," by Jason J. Nassau, McGraw-Hill Book Co., Inc., New York, N. Y., 1932, p. 141.

[11] Capt., U. S. Navy (*Retired*), Annapolis, Md.

[12] "Latitude, Longitude, and Azimuth by the Sun or Stars," by T. F. Hickerson, Chapel Hill, N. C., 1947.

Since Professor Hickerson's explanations for the various problems involved are clear, most of the writer's comments are restricted to the background of the secant tables published and explained by the author. As far as is known, the first condensed tables using log secants multiplied by 10^5 for simplification were devised by S. Ogura, a hydrographic engineer of the Japanese hydrographic department. These tables were a combination of tables of A and B, the table of A being for a quick solution of the right triangle and the table of B for the usual solution. The writer copied the Ogura tables (with permission) in 1927.[13] Subsequent adaptations of these tables include those of A. A. Ageton[14] (which rearranged the secant tables) and other similar tables.

During World War II, as Convoy Commodore, the writer noted that some marine navigators insisted on using the "time sight," whereby the latitude is assumed and the longitude is computed from the declination, the altitude, and the assumed latitude. The method was tested to see why some navigators insisted on its use even though it was longer than other available methods. Part of the reason appeared to be psychological because for an assumed latitude a definite longitude was computed, so that a definite point could be put on the chart. The only trouble with this method is that another assumed latitude gives another longitude, and, if two points are computed, it will give the usual line of position.

While perusing the formula for the time sights, it was noted that the formula could be rearranged so that secants and cosecants could be used throughout. It was decided that a short table would be published permitting the longitude to be computed using only nine pages of secants and cosecants. This is exactly what Professor Hickerson did independently.

The secant tables were available in fairly convenient forms in several publications.[12,13,14] However, it was believed that an article stating that the time sight could be calculated by tables already included in other methods would not impress the marine navigator as much as would a separate book. For this reason the tables[13] published by Naval Institute at Annapolis, Md., were reduced to a table giving nine pages of secants and cosecants conveniently arranged with auxiliary tables.[15] By test, the method proved to be about 7% faster than the older time-sight method, in spite of the fact that the method required more steps.

Obviously the advantage of these methods resides largely in the simplification of the logarithm as published by Professor Hickerson, by the writer, and by others. Without question the young man starting in engineering could, with Professor Hickerson's tables and explanations, determine accurately the values of latitude, longitude, and azimuth, not to mention many other operations.

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[13] "Line of Position Book," by P. V. H. Weems, U. S. Naval Institute, Annapolis, Md., 1927.

[14] "Dead Reckoning Altitude and Azimuth Table," by A. A. Ageton, *Publication No. 211*, Hydrographic Office, U. S. Navy Dept., Washington, D. C., 1942.

[15] "The Secant Time Sight," by P. V. H. Weems, Weems System of Navigation, Annapolis, Md., 1944.

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DISCUSSIONS

VARIATION OF COEFFICIENTS OF SIMULTANEOUS LINEAR EQUATIONS

Discussion

BY I. F. MORRISON, SIGMUND ROOS, AND BERNARD L. WEINER

I. F. MORRISON,[6] ESQ.—In this interesting paper the author has developed a systematic method for the solution of simultaneous linear equations which offers "several marked advantages over the classical methods" without specifying which of the several classical methods. The method developed in the paper somewhat resembles the Doolittle method, which is a modification of the method developed by C. F. Gauss. The writer has always found this to be a most satisfactory method for the solution of such equations. It is certainly classical. In so far as he is aware, no difficulty concerning the small difference of quantities arises, as it so often does in the classical Cramer method. The introduction of the idea of a change in some of the coefficients is novel and of merit. The method developed in the paper possesses the advantages inherent in all well-planned computation procedure.

In the analysis of any structure, indeterminate to the nth degree, a set of simultaneous linear equations eventually must be solved in one way or another. Even the modern relaxation methods are simply a way of doing this. These equations may be written in condensed form as

$$\boldsymbol{a}\,\boldsymbol{x} = \boldsymbol{C} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (40)$$

in which $\boldsymbol{a}$ is a square matrix of order n, based solely on the size, shape, and elastic properties of the structure; in fact, it may be said to represent the structure in mathematical form. The elements of this matrix, a_{ij}, are most conveniently computed numerically from assumed self-stressed conditions. The elements of $\boldsymbol{x}$, which is a column matrix, are the redundants—that is, the unknown thrusts, moments, shears, etc., which are sought.

The elements of $\boldsymbol{C}$, also a column matrix, are the loading terms. They depend on the loads and the structure but, nevertheless, are independent of $\boldsymbol{a}$. They may be changed to meet any number of local requirements.

NOTE.—This paper by Bernard L. Weiner was published in October, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: May, 1948, by T. F. Hickerson, A. Floris, and Frederick S. Merritt.

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The solution of Eq. 40 is

$$x = a^{-1} C \quad \ldots\ldots (41)$$

and since a^{-1} is not dependent on C, it will be constant for all loadings provided there be no change in a.

The paper presents the following mathematical problem: Given a set of simultaneous equations in the form of Eq. 40, in which the symbols represent matrices, which have been solved for x, it is required to find from that solution what new values of x (say, x') will result from a change of the matrix a into $[a + b]$. The assumed solution of Eq. 40 is given by Eq. 41, which presupposes that a^{-1} is known. According to the conditions of the problem,

$$[a + b] x' = C = a x \quad \ldots\ldots (42a)$$

which, multiplied through by a^{-1}, becomes

$$a^{-1} [a + b] x' = [I + a^{-1} b] x' = x \quad \ldots\ldots (42b)$$

In Eq. 42*b*, I is the unit matrix. To solve Eq. 42*b*, multiply through by $[I + a^{-1} b]^{-1}$, so that,

$$x' = [I + a^{-1} b]^{-1} x \quad \ldots\ldots (43)$$

In this solution, all the factors on the right-hand side are known so that x' is determined from the original x.

By way of further explanation, it should be mentioned that the reciprocal matrix is best determined by finding the adjoint of the matrix and dividing by the determinant of the matrix. Thus, Eq. 43 may be written

$$x' = \frac{\text{adjoint } [I + a^{-1} b]}{|I + a^{-1} b|} x \quad \ldots\ldots (44)$$

The adjoint A of a matrix a is another matrix of the same order in which the element a_{ij} is the cofactor of the element a_{ji}. The cofactor of a_{ji} is $(-1)^{i+j}$ times the $(n - 1)$-row determinant obtained by striking out row j and column i of $|a|$. It is not necessary to work out the value of the determinant because, if one multiplies the first row of the matrix by the first column of the adjoint, the result is the numerical value of the determinant. It is advisable to avoid large determinants.

In the first example (which, of course, has been chosen for simplicity), $a = \begin{bmatrix} 4 & 6 \\ 2 & 5 \end{bmatrix}$; $b = \begin{bmatrix} 3 & 0 \\ 0 & 0 \end{bmatrix}$; $a^{-1} = \frac{1}{8}\begin{bmatrix} 5 & -6 \\ -2 & 4 \end{bmatrix}$; and $x = \begin{bmatrix} 3 \\ 5 \end{bmatrix}$. According to the foregoing theory,

$$a^{-1} b = \frac{1}{8}\begin{bmatrix} 5 & -6 \\ -2 & 4 \end{bmatrix}\begin{bmatrix} 3 & 0 \\ 0 & 0 \end{bmatrix} = \frac{1}{8}\begin{bmatrix} 15 & 0 \\ -6 & 0 \end{bmatrix}$$

$$I + a^{-1} b = \frac{1}{8}\begin{bmatrix} 23 & 0 \\ -6 & 8 \end{bmatrix}$$

and

$$[I + a^{-1} b]^{-1} = \begin{bmatrix} \frac{8}{23} & 0 \\ \frac{6}{23} & 1 \end{bmatrix}$$

Thus,

$$\begin{bmatrix} x' \\ y' \end{bmatrix} = \begin{bmatrix} \frac{8}{23} & 0 \\ \frac{6}{23} & 1 \end{bmatrix} \begin{bmatrix} 3 \\ 5 \end{bmatrix} = \begin{bmatrix} \frac{24}{23} \\ \frac{133}{23} \end{bmatrix}$$

These results check those given in the paper. The first row of a-values multiplied by the first column of the cofactor of a^{-1} is 8, which is the value of the determinant $\begin{vmatrix} 4 & 6 \\ 2 & 5 \end{vmatrix}$.

The foregoing theory is quite general, however. Suppose, for example, all the elements of the original matrix are changed so that $b = \begin{bmatrix} 1 & -1 \\ -3 & 2 \end{bmatrix}$; and let the other data remain the same. Then,

$$a^{-1}b = \frac{1}{8}\begin{bmatrix} 5 & -6 \\ -2 & 4 \end{bmatrix}\begin{bmatrix} 1 & -1 \\ -3 & 2 \end{bmatrix} = \frac{1}{8}\begin{bmatrix} 23 & -17 \\ -14 & 10 \end{bmatrix}$$

$$I + a^{-1}b = \frac{1}{8}\begin{bmatrix} 32 & -17 \\ -14 & 18 \end{bmatrix}$$

$$[I + a^{-1}b]^{-1} = \frac{1}{40}\begin{bmatrix} 18 & 17 \\ 14 & 31 \end{bmatrix}$$

and

$$\begin{bmatrix} x' \\ y' \end{bmatrix} = \frac{1}{40}\begin{bmatrix} 18 & 17 \\ 14 & 31 \end{bmatrix}\begin{bmatrix} 3 \\ 5 \end{bmatrix} = \begin{bmatrix} \frac{139}{40} \\ \frac{197}{40} \end{bmatrix}$$

Referring to Eqs. 22 of the paper,

$$\text{Adjoint}\begin{bmatrix} 4 & 3 & 2 \\ 1 & 4 & 3 \\ 5 & 2 & 6 \end{bmatrix} = \begin{bmatrix} 18 & -14 & 1 \\ 9 & 14 & -10 \\ -18 & 7 & 13 \end{bmatrix}$$

from which the reciprocal matrix and solution of the equations can be written immediately.

Whether this general method offers any advantage over a direct solution starting fresh with the revised, or new, matrix is open to doubt. The author has developed a step-by-step process, in work sheet form, which provides periodic checks. It is likely that this method will possess a definite advantage, especially for those not familiar with matrix calculations. For large sets of equations the method of submatrices[7] will be found very useful.

The author has correctly stated that in structural analysis certain coefficients occur in pairs. For any structure, the analysis of which is based on the assumption of Hooke's law, the matrix a, as a consequence, will always be symmetrical. This fact affords a check on computations because the reciprocal matrix a^{-1} will also be symmetrical. Moreover, the product of row 1 of a by

[7] "Some Devices for the Solution of Large Sets of Simultaneous Linear Equations," by W. J. Duncan *Philosophical Magazine*, Vol. 35, 1944, p. 660.

column 1 of a^{-1}, row 2 by column 2, etc., will always be one; and row 1 by column 2, etc., will be zero. This affords a check on the computation of the reciprocal matrix.

SIGMUND ROOS,[8] M. ASCE.—The need for an efficient method of solving simultaneous linear equations is of primary importance in all fields of engineering. The method of variation of coefficients presented in this paper was developed for use in solving the equations encountered in structural engineering work, but it can be used for other equations to great advantage. The method is applied easily and gives exact answers rapidly.

The author is to be commended highly for his work, as he has employed the principles of advanced mathematics to formulate procedures which may be used without knowledge of the underlying mathematics. In this connection, it is suggested that the author add to his closing remarks a rigorous mathematical proof of the method. More engineers would be inclined to use the method if they knew its scientific basis.

It is difficult to make comparisons between the present paper and others that go so far toward satisfying a universal need among all engineers. In the field of structural engineering, however, it will be found that Mr. Weiner's paper has certain distinct advantages. It can be checked at each step and it can be adjusted easily to changes in design.

It is the last mentioned feature of the method of variation of coefficients which makes this method particularly attractive. A system of equations which represents the geometric and elastic properties of a statically indeterminate structure may undergo considerable variation as the problems of design and detail are more completely studied. Minor changes of any one member or group of members of a continuous framework usually have an insignificant effect on the entire structure. An experienced designer knows the limits within which the length and cross section of a member can be varied without changing the elastic properties of the structure as a whole. Nevertheless, these limits are often exceeded, and the present method provides a quick procedure of accurately determining the resultant effects without resolving the entire system of equations. The writer has found the method of variation of coefficients to be a remarkably efficient tool for this purpose. In one case, it was found that, when the foundations for a rigid frame bridge were excavated, the elevation of rock was considerably lower than indicated on the boring record. The necessary adjustments in design were easily performed by using the method presented in this paper.

The method also presents a possibility not mentioned by the author, in that it is possible to use this method for the direct design of statically indeterminate structures. If one frame of a group of similar frames is designed, the other frames may be designed directly by merely varying the coefficients of the system of equations for the first frame.

In addition, the method of variation of coefficients possesses a very dynamic quality. Any system of equations can be extended far beyond the original

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conditions, thus giving the user great freedom of action. For these reasons, many new and important applications will undoubtedly be found as the method is used.

BERNARD L. WEINER,[9] M. ASCE.—It is unfortunate that most of the discussers have stressed only one feature of the paper—the application of the theory to the solution of simultaneous equations—and have compared the method of solution by the variation of coefficients with other methods. The writer has never felt this application to be the salient feature of the theory. In fact, the theory was developed by the writer when it was necessary to find some way of correcting several errors in coefficients in a rather complicated analysis when there was insufficient time to re-solve the equations with the corrected coefficients. Application of the theory to the solution of equations came later as a rather obvious corollary.

As far as the solution of equations is concerned, comparisons may be made between the method of varying coefficients and other methods—both new and old. However, as far as the writer knows, there is no other method for finding the effect on the values of the unknowns resulting from a change in the coefficients—excepting, of course, the obvious one of re-solving the equations. It is well known that the analysis of statically indeterminate structures consists of the process of assuming the sizes of the various members—either from previous experience or from preliminary approximate designs—and then analyzing the structure to check the adequacy of the assumed sizes. If the designer is either sufficiently experienced, or lucky, his first assumption may be close enough for design purposes. However, should he find that his "guesses" are too far off, he must run another complete check analysis to test the revised sections.

With the method presented in this paper, the effect of the changes can be determined without going through an entirely new analysis. For frames and other structures in which the moments are large in comparison to the thrust—that is, in which the thrust has a large eccentricity—rather drastic changes may be made in the sections without seriously affecting the value of the redundants. However, in arches and other structures in which the thrust has a small eccentricity, the stresses are sensitive to changes in section and a check analysis becomes imperative. Variation of coefficients is a valuable tool in such cases.

However, in most instances, even if the designer finds that his first assumption of member sizes is sufficiently close, he cannot be sure that he has achieved the maximum in economy unless he tries to change sizes to achieve a net reduction in material. This, of course, involves changing the coefficients. Such studies, in which the number of redundants is large, are too time consuming to permit anything more than a very rough and inadequate investigation if the equations are re-solved for every change.

The writer believes that this application of the paper is its most important feature. Although it is believed that the theory offers many advantages in its application to the solution of equations, there are other methods. The fact remains, however, that variation of coefficients is the only known quick method for making the investigations previously described.

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It is to be noted that, for the variation of any number of the coefficients of one unknown, only two simple formulas are necessary for the application of the theory. The factors such as u_d and v_d are actually solutions of the equations for a set of constants $c_n = d_1, d_2, d_3, \cdots, d_n$. Hence, no cumbersome formulas are necessary for their evaluations. This fact makes the theory easy to apply and the procedure also lends itself readily to work sheet form.

The writer disagrees with Mr. Floris' comment that Col. 7, Table 1, headed "Remarks," proves that the method of variation of coefficients is a modification of the process of successive elimination of coefficients. Table 1 is merely introductory and its heading states that it gives the solution of Eqs. 2 by the process of elimination. The purpose of this table is to obtain a solution by any method whatever, in order to illustrate the properties of certain solutions—namely, solutions for the values of $c_n = a, 0, 0; 0, b, 0;$ and $0, 0, c$. Later in the paper, it is shown that these solutions can be used to obtain the necessary conversion factors for varying the coefficients and are therefore designated "variation coefficients."

Mr. Floris states that the theory is not readily adaptable to the solution of certain types of equations and gives Eqs. 38 as an example. These equations are rather "tricky" and their solution would be difficult by any method unless special devices were used. This situation occurs fairly often in computations when the equations under consideration approach certain limits. Under such conditions, if certain special methods are used, the solution becomes very simple—more simple, very often, than in more "normal" cases; if they are not used, the solution becomes very difficult. A common example is the problem of finding middle ordinates or tangent offsets to a circular arc with very large radius. The usual method of the difference of the squares becomes very cumbersome; successive approximations however make the solution very simple—so simple in fact that slide rule computations are often sufficiently accurate. If the radius is very large, even the first correction is small enough to be neglected.

Mr. Floris shows that Eqs. 38 have the solution $x = y = z = u = 1$, but states that they also have other solutions. He then gives a series of solutions and shows that substitution of these values in Eqs. 38 results in a series of constants on the right-hand side of the equations which differ only slightly from the original constants.

The writer must disagree with Mr. Floris' statement that Eqs. 38 are satisfied by more than one value for each unknown. The determinant formed from the coefficients of Eqs. 38 is equal to 1. Since this determinant does not equal zero, it follows from the theory of equations that there exists only one solution for each unknown for any given set of constants. In spite of the fact that the values of the constants given in the second set of numbers differ only slightly from the original values (23.1, 23.01, and 23.001 instead of the original 23, for example), these constants are not the same, as would be the case if it were merely a question of accuracy of computations. The values given are absolutely exact and are different sets of constants. The difficulty lies in the fact that the equations are nearly "parallel" to each other and are therefore sensitive to slight changes—in this region, at least.

If similar equations with only two unknowns were solved, the solution would represent the coordinates of the intersection of two straight lines. If these lines were nearly parallel, their intersection point would be very sensitive to the constants on the right-hand side of the equations. Properly transformed —as shown in analytical geometry—these constants could be made to represent the distance from the origin of the points where the lines intersect the x-axis. If the two lines were made more and more nearly parallel, a slight change in this distance for one of the lines would make a tremendous difference in the point of the intersection. A change of, say, one unit in this distance could be made to result in a change of a million units—or any other large number in the location of the point of intersection by making the lines sufficiently parallel to each other. If they were absolutely parallel, the change, of course, would be infinity.

This conclusion can readily be proved. If Eqs. 38 are solved for a set of constants,

$$c_n = 1, -1, -1, 1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (45)$$

the results are

$$x = 136, \quad y = -82, \quad z = -35, \quad \text{and} \quad u = 21 \ldots\ldots\ldots (46)$$

Mr. Floris' results for the new constants of Eqs. 38 can be obtained by superposition by combining the original constants with multiples of Eq. 45. Combining Eq. 39 with the same multiples of Eqs. 46, the corresponding new values for x, y, z, and u given by Mr. Floris can be obtained; for example, taking 0.1 of Eq. 45 and adding to the constants of Eqs. 38 gives: $23 + 0.1 = 23.1$; $32 - 0.1 = 31.9$; $33 - 0.1 = 32.9$; and $31 + 0.1 = 31.1$.

Taking 0.1 of Eqs. 46 and combining them with Eq. 39 gives: $x = 1 + 0.1 \times 136 = 14.6$; $y = 1 - 0.1 \times (-86) = -7.6$; $z = 1 + 0.1 \times (-35) = -2.5$; and $1 + 0.1 \times 21 = 3.1$.

The remaining values on the second and third lines of the two sets of numbers given by Mr. Floris may be similarly obtained by using factors 0.01 and 0.001, respectively, instead of the factor 0.1 used in this computation.

This computation clearly illustrates that Eqs. 38 can be solved for any set of constants and the solution will have only one value for each unknown. The method of variation of coefficients may be used as well as any other method. This example of "ill-conditioned equations," as Mr. Floris calls them, is susceptible to easy solution with the aid of special devices (as are other equations) when they approach certain limits which make them difficult to handle by "routine" computations.

The contention of Mr. Floris that variation of coefficients is of limited usefulness is contradicted by some of the other discussers, especially by Mr. Roos who tells of some applications of the theory to problems he has solved.

Professor Hickerson varies the coefficients of the writer's Eqs. 22 as illustrated in Table 4 by re-solving the equations by successive elimination of the unknowns. The work is illustrated in Table 10; and, by using a, b, and c for the constants in addition to the numerical values 28, 26, and 48, it is shown how the basic formulas (Eqs. 21) come into the computation. The letter constants, a, b, and c, give rise, of course, to the solutions designated by the writer

as "variation coefficients." After completing the solution, this method leaves the computer no advantage whatever. He has to re-solve the equations once more for another variation—unless he takes advantage of the variation coefficients as explained in the paper.

Professor Hickerson also solves Eqs. 35, by successive elimination of the unknowns. The advantage of breaking up a large set of equations into smaller sets as was done by the writer is not apparent in this example for two reasons. Throughout the paper, simple numerical examples were chosen in order to illustrate principles, thus eliminating the distractions produced by the mechanical difficulty of handling large numbers. Secondly, four equations with four unknowns only begin to make the solution of such equations cumbersome. However, as the number of terms in a set of simultaneous equations is equal to the square of the number of unknowns, this number increases very rapidly. In larger sets of equations (equations with twelve and eighteen unknowns) the simplifications become very marked.

The writer notices that Professor Hickerson has omitted the "Remarks" column from his work sheets. The writer on numerous occasions has found this column invaluable and has been grateful to the unknown person who suggested its use. The writer agrees wholeheartedly with one of the other discussers (Professor Morrison) that systematic arrangement is the very essence of any computation. It not only helps eliminate errors, but also reduces the labor involved in any computation more than practically any other one single feature. The writer has made a statement (under the heading, "Introduction") to this effect in which it is also stated that the computation for multiple loading constants (such as influence lines) is no great hardship if the work is systematically arranged. The writer fails to see how anything but numerous loading conditions can be used in the design of modern structures.

Since the computation of variation coefficients is a simple matter if the work is systematically arranged, the device suggested by Professor Hickerson for varying the coefficients of at least one unknown by re-solving becomes unnecessary. In fact, the writer sees no need at all for re-solving the equations. That 50% or more of the work would be saved by omitting the constants a, b, and c is greatly to be doubted in view of the arguments previously given. The question whether or not to use $a, b, c, \cdots, n$, therefore becomes academic, and their use amounts to computing "variation coefficients." In any case, if the constants are "varied" (in Professor Hickerson's words) there is no problem—the procedure for accomplishing this is well known. If this were the only question involved, this paper would never have been written. If the coefficients are varied, there is a problem which this paper has attempted to solve. From either point of view, therefore, it is desirable to compute the variation coefficients.

Professor Morrison's discussion is very interesting from several points of view. Most important, in the writer's judgment, is the point which this discusser makes as to the value of a well-planned computation procedure.

It cannot be reiterated too often that, just as it is necessary to plan a construction job in the field, it is also necessary to plan the work in the office. It has been the writer's sad experience to have to wade through computations spread over innumerable pages, without page numbers, dates, or titles. The

usual result is that such computations are worthless a week or even a day after they are made. They are unintelligible even to the computer—if he has not neglected to add his initials and can be identified. More often than not, such haphazard computations contain serious errors. Wherever possible, computations should be tabulated, thus saving much repetition of explanatory matter. Tabulated computations are concise, are easy to follow, save much labor, and reduce errors. If a series of results is in tabular form, an error can very often be detected simply by the fact that it looks erratic—out of proportion—when compared with similar results adjacent to it.

The writer notes that Professor Morrison considers the idea of a change in some of the coefficients as novel and of merit. To writer's knowledge, this idea is unique in the theory of this type of equations. Professor Morrison develops the fundamental mathematical basis of the paper by the use of matrix algebra and gives the final result in general terms in Eq. 44. It is well worth the effort to follow through this discussion in detail, but, as matrix algebra is not too familiar a subject, a simple explanation of the high lights—sufficient to enable one to follow Professor Morrison's mathematics—is readily available in a paper by Stanley U. Benscoter, Assoc. M. ASCE.[10]

Professor Morrison then proceeds to apply Eq. 44 to some of the problems worked out in the paper; in one case, he solves a problem in which he varies the coefficients of two unknowns. He admits, however, that it is open to doubt whether this general method has any advantage over the direct solution offered by the writer, and again calls attention to the fact that the method presented in the paper lends itself to a step-by-step process in work sheet form, always a distinct advantage in itself.

The writer, too, doubts that this general method based on matrix algebra has any advantages for numerical solutions. For research purposes, however, the method is invaluable. Every time a familiar analysis is studied from a new point of view or by another method of attack, the results will often appear in a new form, hitherto unknown. By making the proper algebraic or other mathematical transformations, the new form can always be converted to the more familiar forms, but a gain has nevertheless been made. Such studies are usually worth the effort.

For this reason, and also to anticipate a point raised by Mr. Roos, the writer has extended Professor Morrison's analysis (Eq. 44) to derive Eqs. 17, 19, and 20, which, when generalized, give Eqs. 21, the basic equations of the paper. Extending the analysis further, the writer has also derived the equations used in multiple variations (Eqs. 33 and 34).

A system of n equations with n unknowns takes the form:

$$\left.\begin{aligned} a_{11}x + a_{12}y + a_{13}z + + + &= c_1 \\ a_{21}x + a_{22}y + a_{23}z + + + &= c_2 \\ a_{31}x + a_{32}y + a_{33}z + + + &= c_3 \\ \cdot\ \cdot\ \cdot

It is desired to increase the coefficients of x by $d_1, d_2, d_3, \cdots, d_n$, respectively, and to find the resulting effect on all the unknowns.

For the present purpose, it will be sufficient to take three equations with three unknowns; the derivation will be exactly the same for any number of unknowns.

Referring to Eq. 44,

$$a = \begin{vmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (48)$$

$$a^{-1} = \frac{\text{adjoint } a}{a} = \frac{1}{a}\begin{vmatrix} A_{11} & A_{21} & A_{31} \\ A_{12} & A_{22} & A_{32} \\ A_{13} & A_{23} & A_{33} \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots (49)$$

and

$$b = \begin{vmatrix} d_1 & 0 & 0 \\ d_2 & 0 & 0 \\ d_3 & 0 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (50)$$

Therefore,

$$a^{-1} b = \frac{1}{a}\begin{vmatrix} A_{11} & A_{21} & A_{31} \\ A_{12} & A_{22} & A_{32} \\ A_{13} & A_{23} & A_{33} \end{vmatrix} \begin{vmatrix} d_1 & 0 & 0 \\ d_2 & 0 & 0 \\ d_3 & 0 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots (51)$$

and

$$a^{-1} b = \frac{1}{a}\begin{vmatrix} (d_1 A_{11} + d_2 A_{21} + d_3 A_{31}) & 0 & 0 \\ (d_1 A_{12} + d_2 A_{22} + d_3 A_{32}) & 0 & 0 \\ (d_1 A_{13} + d_2 A_{23} + d_3 A_{33}) & 0 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots (52)$$

However,

$$\left.\begin{aligned} \frac{d_1 A_{11} + d_2 A_{21} + d_3 A_{31}}{a} &= x_d \\ \frac{d_1 A_{12} + d_2 A_{22} + d_3 A_{32}}{a} &= y_d \\ \frac{d_1 A_{13} + d_2 A_{23} + d_3 A_{33}}{a} &= z_d \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (53)$$

Substituting Eqs. 53 in Eq. 52,

$$a^{-1} b = \begin{vmatrix} x_d & 0 & 0 \\ y_d & 0 & 0 \\ z_d & 0 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (54)$$

and

$$I + a^{-1} b = \begin{vmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{vmatrix} + \begin{vmatrix} x_d & 0 & 0 \\ y_d & 0 & 0 \\ z_d & 0 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots (55a)$$

from which

$$I + a^{-1} b = \begin{vmatrix} 1 + x_d & 0 & 0 \\ y_d & 1 & 0 \\ z_d & 0 & 1 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (55b)$$

and

$$[I + a^{-1} b]^{-1} = \frac{\text{adjoint } [I + a^{-1} b]}{I + a^{-1} b} = \frac{\text{adjoint } [I + a^{-1} b]}{1 + x_d} \quad \ldots\ldots (56a)$$

Therefore,

$$[I + a^{-1} b]^{-1} = \frac{1}{1 + x_d} \begin{vmatrix} 1 & 0 & 0 \\ -y_d & 1 + x_d & 0 \\ -z_d & 0 & 1 + x_d \end{vmatrix} \quad \ldots\ldots\ldots (56b)$$

and

$$\begin{vmatrix} x' \\ y' \\ z' \end{vmatrix} = \frac{1}{I + x_d} \begin{vmatrix} 1 & 0 & 0 \\ -y_d & 1 + x_d & 0 \\ -z_d & 0 & 1 + x_d \end{vmatrix} \begin{vmatrix} x \\ y \\ z \end{vmatrix} \quad \ldots\ldots\ldots\ldots (57)$$

from which

$$\left.\begin{aligned} x' &= \frac{x}{1 + x_d} \\ y' &= y - \frac{y_d\, x}{1 + x_d} \\ z' &= z - \frac{z_d\, x}{1 + x_d} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (58)$$

Eqs. 58 will be recognized as being the same as Eqs. 17, 19, and 20, respectively.

To derive Eqs. 33 and 34, the value of b in Eq. 50 becomes

$$b = \begin{vmatrix} d_1 & e_1 & 0 \\ d_2 & e_2 & 0 \\ d_3 & e_3 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (59)$$

and

$$a^{-1} b = \frac{1}{a} \begin{vmatrix} A_{11} & A_{21} & A_{31} \\ A_{12} & A_{22} & A_{32} \\ A_{13} & A_{23} & A_{33} \end{vmatrix} \begin{vmatrix} d_1 & e_1 & 0 \\ d_2 & e_2 & 0 \\ d_3 & e_3 & 0 \end{vmatrix} \quad \ldots\ldots\ldots\ldots (60)$$

Therefore,

$$I + a^{-1} b = \begin{vmatrix} 1 + x_d & x_e & 0 \\ y_d & 1 + y_e & 0 \\ z_d & z_e & 1 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots (61)$$

$$[I + a^{-1} b]^{-1} = \frac{\begin{vmatrix} (1+y_e) & -x_e & 0 \\ -y_d & (1+x_d) & 0 \\ y_d\, z_e - (1+y_e)\, z_d & x_e\, z_d - (1+x_d)\, z_e & (1+x_d)\,(1+y_e) - x_e\, y_d \end{vmatrix}}{(1+x_d)\,(1+y_e) - x_e\, y_d} \quad \ldots\ldots (62)$$

and

$$\begin{vmatrix} x' \\ y' \\ z' \end{vmatrix} = \frac{1}{D} \begin{vmatrix} (1+y_e) & -x_e & 0 \\ -y_d & (1+x_d) & 0 \\ y_d\, z_e - (1+y_e)\, z_d & x_e\, z_d - (1+x_d)\, z_e & (1+x_d)\,(1+y_e) - x_e\, y_d \end{vmatrix} \begin{vmatrix} x \\ y \\ z \end{vmatrix} \quad \ldots\ldots (63)$$

in which $D = (1 + x_d)\,(1 + y_e) - x_e\, y_d$. Therefore,

$$\left.\begin{aligned} x' &= \frac{(1 + y_e)\, x - x_e\, y}{(1 + x_d)\,(1 + y_e) - x_e\, y_d} \\ y' &= \frac{(1 + x_d)\, y - y_d\, x}{(1 + x_d)\,(1 + y_e) - x_e\, y_d} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots (64)$$

$$z' = z - z_d\, x' - z_e\, y' \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (65)$$

Eqs. 64 are the solutions of Eqs. 29 and 30, and Eq. 65 is the same as Eq. 31.

If all the coefficients were varied at once, Eqs. 50 would become

$$b = \begin{vmatrix} d_1 & e_1 & f_1 \\ d_2 & e_2 & f_2 \\ d_3 & e_3 & f_3 \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (66)$$

and

$$I + a^{-1} b = \begin{vmatrix} 1 + x_d & x_e & x_f \\ y_d & 1 + y_e & y_f \\ z_d & z_e & 1 + z_f \end{vmatrix} \quad \ldots\ldots\ldots\ldots\ldots (67)$$

The matrix of Eq. 67 is a full one; that is, it contains no zeros and is equivalent to the matrix involved in re-solving the equations.

It is again shown that, if the coefficients of all the unknowns are varied at one time, this procedure simply results in the complete solution of the equations by one of the classic methods—in this case, by determinants. In fact, this is exactly the result of Professor Morrison's numerical example illustrating the general solution. The simplicity of this numerical general solution is deceiving; the examples used both by the discusser and by the writer were chosen deliberately for the purpose of illustrating principles with the least amount of distraction from the mechanical details of manipulating difficult numbers.

For this reason the writer agrees with Professor Morrison that it is doubtful whether the general method based on matrix algebra offers any advantage since it is really a solution by determinants. The writer is grateful to Professor Morrison for submitting his very interesting and enlightening discussion.

Mr. Roos' comments relating how he has used the theory presented in this paper indicate that variation of coefficients is not a mere mathematical curiosity, but a useful engineering tool. When the millennium comes, there may be a paradise in which designers will have the foresight to anticipate all contingencies and the final design will be exactly the same as the preliminary design. Adjustments will then not be necessary. The fact remains, however, that normally changes are made and adjustments are necessary.

If the structure under consideration is indeterminate to a large degree, an accurate determination of the exact effect of the changes is too time consuming to permit anything more elaborate than a "guesstimate" to be made. Mr. Roos corroborates the writer's experience that variation of coefficients is an efficient tool for this purpose and for the other purposes he mentions.

Mr. Roos states that, with the method, he has been able to adapt one basic design to several other structures. The writer has stated (under the heading, "Solution of Simultaneous Equations by Variation of Coefficients") that if a solution of any set of equations is known—from some previous computation or in any other way—it can be varied to any other set of equations desired. Thus, the writer, from his own experience, has anticipated Mr. Roos' findings.

Mr. Roos requests the mathematical basis for the method. It is supposed that what is desired is a more rigorous proof, but the writer feels that the proof given under the heading, "General Formulas for Variation of Coefficients," leading to the derivation of Eqs. 21, is sufficiently rigorous. Nevertheless, he has complied with Mr. Roos' request by extending Professor Morrison's Eq. 44 to the derivation of Eqs. 21, 33, and 34. Matrix algebra deals of course

with determinants, hence this latter proof in terms of determinants constitutes a rigorous mathematical proof.

The writer is grateful to Mr. Roos for presenting evidence that the paper is a useful engineering tool and not a mere problem in abstract mathematics.

Mr. Merritt has also used the theory of the paper and calls attention to the fact that, when any of the properties of the structure are changed, resulting in a change in coefficients, classical methods permit no other alternative than re-solving the equations. If the loading conditions are changed, no particular problem is involved—provided the work is arranged according to a well-laid plan. The writer is grateful also to Mr. Merritt for his brief discussion presenting additional evidence of the practical value of the method as an engineering tool.

The writer feels that there are simplifications still possible in the evaluation of simultaneous equations and that further studies will reveal them. One difficulty encountered in the study of these equations is the fact that, when expanded, the algebraic forms rapidly become very cumbersome. On the other hand, if the solutions are expressed symbolically, it is very difficult to see relationships not already known. Matrix algebra and similar devices have been developed to overcome the first difficulty and the writer feels that further study of the theory of variation of coefficients with the aid of such methods should reveal other simplifications. Although modern procedures are most helpful in practical design of structures having a high degree of indeterminancy, the general methods necessitating sets of linear simultaneous equations are still the primary ones for research. It is to be hoped that such studies will be undertaken by the members of the profession and that further simplifications will be discovered and reported.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

A CENTROIDAL METHOD OF RIGID-FRAME ANALYSIS

Discussion

BY L. E. GRINTER, ALEXIS L. GLUCKMANN, YI-MAI YAO, AND PHIL M. FERGUSON

L. E. GRINTER,[2] M. ASCE.—The many methods of frame analysis should not be looked upon as so many unrelated theories. Actually, the basic principles of ring analysis are few. By comparing the centroidal procedure with other methods, such as that involving the use of Maxwell's equations and the neutral-point method, one sees that the author's procedure involves the use of common principles.

The objective of avoiding the need for solving Maxwell's simultaneous equations is inherent in all modern tools of ring analysis. The concept of applying the reactions at the elastic center and of carrying them down to the abutments by one or more rigid brackets was the first method used for eliminating simultaneous equations. To accomplish this result, the reactions are applied at the centroid of the $\frac{ds}{E\,I}$-values of the several parts or divisions of the structure. This centroid has been termed the neutral point as well as the elastic center.

The principal variation of the neutral-point method has been the concept of the column analogy introduced by Hardy Cross,[3,4] M. ASCE. The column analogy offers nothing not possible through the neutral-point method, since the two have identical root derivations—that is, both simplify Maxwell's equations by choosing such coordinate axes that all except two terms in each of the three simultaneous equations become zero and the three simultaneous equations reduce to three independent equations. The usefulness of the column analogy is its helpfulness in organizing calculations, eliminating confusion over signs, and reducing the intermediate consideration of reaction forces when moments are needed for design. It is possible, therefore, to state categorically

NOTE.—This paper by S. J. Bell was published in November, 1947, *Proceedings*.

[2] Research Prof. of Civ. Eng. and Mechanics, Illinois Inst. of Technology, Chicago, Ill.

[3] "The Column Analogy," by Hardy Cross, *Bulletin No. 215*, Univ. of Illinois Eng. Experiment Station, Urbana, Ill., 1930.

[4] "Theory of Modern Steel Structures," by L. E. Grinter, The Macmillan Co., New York, N. Y., 1937 Vol. 2, pp. 206 and 216.

that the author's centroidal method can accomplish no more than the neutral-point method or the column analogy. However, its usefulness is to be judged on the basis of such important considerations as clarity or ease of understanding, convenience in making routine calculations, and visual characterstics that aid one both to understand and to remember the procedures. For clarity of the physical picture, the writer has always considered the use of neutral-point forces to be desirable, but anyone who has used the column analogy knows that it saves much time and reduces the probabilities of clerical errors.

The author's procedure is more closely akin to the older neutral-point method than to the column analogy. In fact, it is a neutral-point method in which use is made of one resultant reactive force which takes the place of the thrust, shear, and moment reactions at the neutral point. In Fig. 9, for example, the eccentric force F_o of 8,180 lb produces a moment of 8,180 × 27.5 in. or 225,000 in.-lb about the neutral point (centroid of the $\frac{ds}{E\,I}$-values). The same neutral-point moment is obtained by dividing the rotation at the neutral point for the static structure, $\int M_s \frac{ds}{E\,I}$, by the rotation at the neutral point due to unit neutral-point moment, $\int \frac{ds}{E\,I}$. Hence,

$$M_o = \frac{\int M_s \frac{ds}{E\,I}}{\int \frac{ds}{E\,I}} = \frac{0.5 \times 1{,}800{,}000 \times 180 \div 900}{\frac{180}{900} + \frac{120}{600} + \frac{120}{300}} = 225{,}000 \text{ in.-lb.}$$

Evidently, as soon as M_o, H_o, and V_o are computed by the neutral-point method, the value of F_o may be obtained as $\sqrt{H^2_o + V^2_o}$, its inclination may be found from $\tan \theta = V_o/H_o$, and its eccentricity from the neutral point is M_o/F_o.

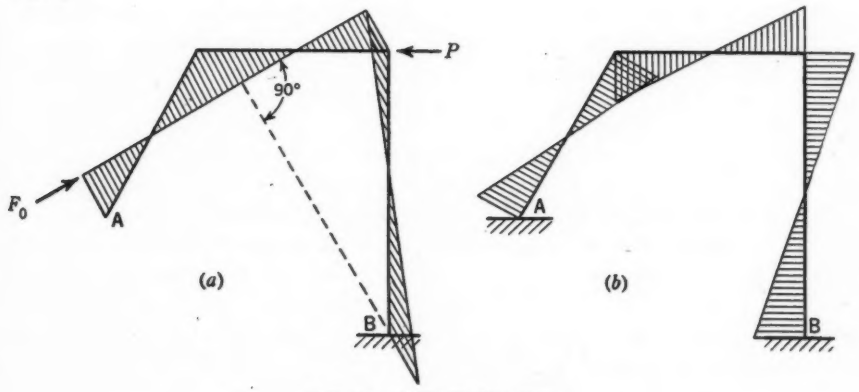

FIG. 18.—MOMENT DIAGRAMS
(*a*) Plotted on F_o, Ordinates Normal to F_o
(*b*) Plotted on Compressive Side, Normal to Each Member

The question is in reality whether it is more effective to use a resultant eccentric force, F_o, than its neutral-point components H_o, V_o, and M_o. As may be noted from the author's illustrations, one of which is reproduced in part as

Fig. 18, there is a certain pictorial quality to the moment diagram drawn on the force F_o as a base line. It is a device that may be used qualitatively in an effective manner. However, even in Fig. 18(*a*), one notices that the device does not add clarity but rather, in fact, is somewhat confusing when applied to drawing the moment diagram for the right-hand column. The ordinary type of bending moment diagram shown in Fig. 18(*b*) seems preferable.

It appears that the method described is in all ways a direct alternative to the common procedure of the neutral-point method. Its distinguishing characteristic is that it gives directly the resultant eccentric force in place of its neutral-point components. This force may then be used to determine graphically a moment diagram plotted on the eccentric force as a base line, all ordinates being normal thereto. The author's procedure might conceivably replace the neutral-point method, but not the column analogy, which offers quite a different set of advantages. The time needed to analyze any ring structure by the author's centroidal procedure with a single reactive force will not be appreciably different from that required for previously known variations of the neutral-point method.

ALEXIS L. GLUCKMANN,[5] ESQ.—The methods of analysis of rigid frames exposed in this interesting paper result directly from the properties of the ellipse of elasticity as shown by M. Marbec, in 1905.[6] For frames of uniform cross section his results are similar to those of Mr. Bell. Among other helpful short cuts, Mr. Marbec gave the following very practical rule for computing the centers of gravity, the statical moments, the moments of inertia, and the centrifugal moments of straight lines and of circular arcs with a central angle $\alpha \leqq \frac{\pi}{2}$:

Assume 1/6 of the total mass of the straight line (or of the circular arc) to be concentrated at each end; and assume the remainder, or four sixths of the total mass to be concentrated at the middle point of the straight line (or of the arc). These assumptions, used for computing the centers of gravity, the statical moments, and the moments of inertia, give an exact result for the straight line and an approximate result for the circular arc; the approximations are sufficient for slide rule computations.

The writer has found that, for this type of problem, and especially for frames with given displacements of both supports A and B, it is often convenient to use the deformation equations of curved beams as established by J. A. C. Bresse in 1854.[7] For a frame without directly applied loads, the bending moment can be written

$$M = a\,x + b\,y + m \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (29)$$

To compute the three unknowns m, a, and b, the following formulas, deduced from those of Mr. Bresse, may be used

$$-\,m \int_A^B \frac{ds}{E\,I} = \theta_B - \theta_A \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (30)$$

[5] Asst. Designing Engr., Gibbs and Hill, Inc., New York, N. Y.

[6] "Étude sur la Dilatation des Tuyautages," by M. Marbec, Ministère de la Marine, Memorial du Génie Maritime, 3me Serie, Fascicule VII, Paris, 1905.

[7] "Recherches Analytiques sur la Flexion et la Résistance des Pièces Courbes," by Jacques Antoine Charles Bresse, Mallet-Bachelier, Paris, 1854.

$$b\int_A^B y^2\,\frac{ds}{E\,I} = \Delta^B{}_x - \Delta^A{}_x - \theta_B\,y_B + \theta_A\,y_A \quad \ldots\ldots\ldots\ldots (31)$$

and

$$-\,a\int_A^B x^2\,\frac{ds}{E\,I} = \Delta^B{}_y - \Delta^A{}_y + \theta_B\,x_B - \theta_A\,x_A \quad \ldots\ldots\ldots\ldots (32)$$

in which x and y are the coordinates of the center line of the frame with respect to its principal axes; x_A, y_A, x_B, and y_B are the coordinates of the supports A and B with respect to the same axes; and θ_A, θ_B, $\Delta^A{}_x$, $\Delta^B{}_x$, $\Delta^A{}_y$, and $\Delta^B{}_y$ are the given displacements of the supports. The integrals of Eqs. 30, 31, and 32 have an obvious meaning and, in all cases, can be computed easily with the help of Marbec's rule previously given.

YI-MAI YAO,[8] ESQ.—The theory of the semigraphical method, offered by Mr. Bell, is sound and is similar in character to the column analogy method.[3] The equivalent items are shown in Table 1.

TABLE 1.—COMPARISON OF EQUIVALENT ITEMS

Item	Centroidal Method		Column Analogy Method	
	Name	Notation	Name	Notation
(1)	(2)	(3)	(4)	(5)
Moment	Moment	M	Stress	$f = M\,i$
Angular displacement...	Angular displacement	$\phi = \Sigma\theta$	Total load	$P = \Sigma\theta$
$\int\frac{ds}{I}$.....	Structural length	$L_o = \int\frac{ds}{I}$	Area	$A = \int\frac{ds}{I}$
Linear displacement....	Component of Δ_o parallel to x-axis	Δ_{xo}	Moment with respect to x-axis	M_{x-x}
	Component of Δ_o parallel to y-axis	Δ_{yo}	Moment with respect to y-axis	M_{y-y}
Moment of inertia.....	Moment of inertia with respect to x-axis	I_x	Moment of inertia with respect to x-axis	I_x
	Moment of inertia with respect to y-axis	I_y	Moment of inertia with respect to y-axis	I_y
Thrust line.....	Thrust line	....	Neutral axis	....

There is no advantage in locating the thrust line by use of Mohr's circle as, in many instances, I_x and I_y are almost equal and the "circle" will give an inaccurate result. The additional equations are derived as follows:

For a rigid frame with variable moment of inertia,

$$F_o = \sqrt{F^2{}_x + F^2{}_y} \quad \ldots\ldots\ldots\ldots (33)$$

in which

$$F_x = \frac{\Delta_{xo}\,E}{I_x} \quad \ldots\ldots\ldots\ldots (34a)$$

and

$$F_y = \frac{\Delta_{yo}\,E}{I_y} \quad \ldots\ldots\ldots\ldots (34b)$$

Eqs. 34 are similar in form to Eqs. 15.

[8] Junior Engr., TangKu New Harbor Construction Bureau, TangKu, Tientsin, China.

In the column analogy method, the stress at any point on the analogous column section can be computed from the simple equation for an eccentrically loaded column—

$$f = \frac{P}{A} \pm \frac{M_{x-x}\, y}{I_x} \pm \frac{M_{y-y}\, x}{I_y} \quad \ldots\ldots (35)$$

From Eq. 35, if $f = 0$ and $y = 0$,

$$x_1 = \frac{P\, I_y}{A\, M_{y-y}} \quad \ldots\ldots (36a)$$

and, if $f = 0$ and $x = 0$,

$$y_1 = \frac{P\, I_x}{A\, M_{x-x}} \quad \ldots\ldots (36b)$$

By substitution of Eqs. 34*a* and 34*b* in Eqs. 36*a* and 36*b*, respectively, then

$$x_1 = \frac{\phi\, I_y}{L_o\, \Delta_{yo}} = \frac{\phi\, E}{L_o\, F_y} \quad \ldots\ldots (37a)$$

and

$$y_1 = \frac{\phi\, I_x}{L_o\, \Delta_{xo}} = \frac{\phi\, E}{L_o\, F_x} \quad \ldots\ldots (37b)$$

Such equations can be derived independently, of course, as was Eq. 6.

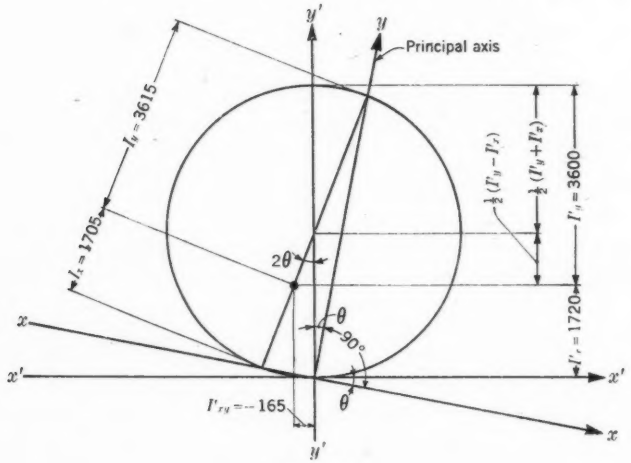

FIG. 19

The distance from the centroid to the thrust line can be easily calculated by geometry; that is,

$$Z_o = \frac{x_1\, y_1}{\sqrt{x^2_1 + y^2_1}} \quad \ldots\ldots (38)$$

By use of Eqs. 37, the thrust line can be located easily and accurately.

For Example 2, Mohr's circle as shown in Fig. 5 may be omitted. The quantities I_x, I_y, and the rotation angle θ to the principal axes can be obtained by another form of Mohr's circle,[9] as shown in Fig. 19.

[9] "Über die Bestimmung und die graphische Darestellung von Trägheitsmomenten ebener Flächen," by Otto Mohr, *Zivilingenieur*, 1887, Vol. 33, p. 43.

From Fig. 19 and the displacement diagram (Fig. 20(*b*)), $\Delta_{xo} = 0.485$ in., $\Delta_{yo} = 0.244$ in., $I_x = 1{,}705$ in.^{4}, and $I_y = 3{,}615$ in.^{4} Substituting these values in Eqs. 34*a*, 34*b*, 37*a*, and 37*b* gives $F_x = \dfrac{(0.485)\ (30 \times 10^6)}{1{,}705} = 8{,}520$ lb, $F_y = \dfrac{(0.244)\ (30 \times 10^6)}{3{,}615} = 2{,}022$ lb, $x_1 = \dfrac{(0.0032)\ (30 \times 10^6)}{(0.8)\ (2{,}022)} = 59.40$ in., and

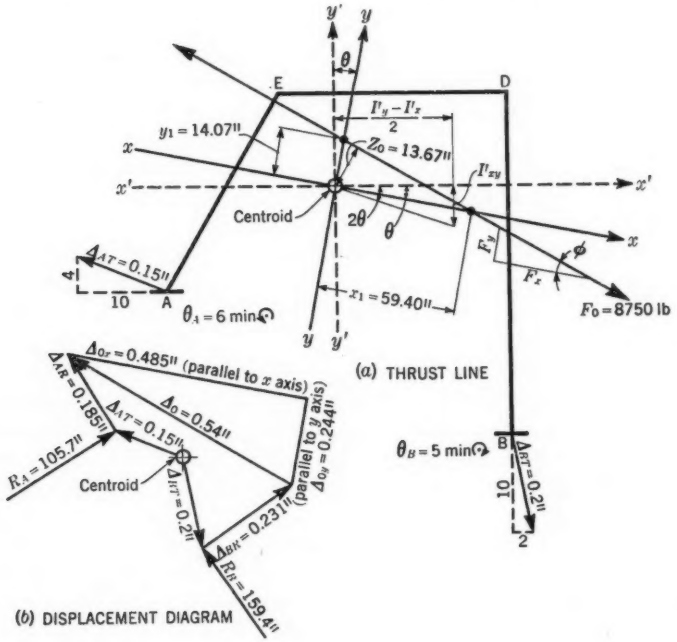

FIG. 20

$y_1 = \dfrac{(0.0032)\ (30 \times 10^6)}{(0.8)\ (8{,}520)} = 14.07$ in., from which the thrust F_o can be located as shown in Fig. 20 and is found to be $F_o = \sqrt{8{,}520^2 + 2{,}022^2} = 8{,}750$ lb. The distance from the centroid to the thrust is $Z_o = \dfrac{(59.40)\ (14.07)}{\sqrt{59.40^2 + 14.07^2}} = 13.67$ in. The value Z_o is useless in analysis, and is only used here as a check on the value computed by the method of the paper.

PHIL M. FERGUSON,[10] M. ASCE.—Engineering literature is enriched and the viewpoint of engineers is widened by papers of this type, which indicate fresh theories and methods derived from particular problems that have faced the author. The majority of structural engineers have regarded actual movements of supports of rigid frames as more or less incidental problems, often limited to relatively simple temperature changes. Their primary problem has

[10] Prof. of Civ. Eng., Univ. of Texas, Austin, Tex.

been an analysis for external loads. Hence, they have adopted methods particularly suited to external loading and have managed to apply these methods to such temperature deformations or settlements as they wished to assume. Mr. Bell's method comes out of a situation where movements of supports are the basic data and lack of symmetry is common. It is the writer's opinion that the proposed method is most useful in this area, although it can be extended, as the author shows, to external loads.

The ellipse of elasticity and the column analogy also lend themselves to this type of calculation. The ellipse of elasticity is seriously handicapped by its reliance on the geometry of the ellipse, this geometry not being generally familiar to American engineers. On the other hand, the column analogy also labors under handicaps: First, the concept whereby a couple acting on the analogous column represents a linear movement of one end of the frame is thoroughly artificial; and, second, the signs of the couples representing linear displacements become particularly confusing when they must be related to movements of both supports. Those who use the column analogy frequently will not find this handicap serious, but signs seem less of a problem with Mr. Bell's method. The two methods seem about equally usable for stiffness and carry-over determinations.

For those who particularly like a graphical as compared to an analytical solution, Mr. Bell's method should have a wider range of appeal because analytical work is reduced to a minimum. The combination of the classical circle of inertia method for determining the principal axes and the further use of this same circle in the solution for F_o leads to economy of time and reduction in errors.

Although those to whom Mr. Bell's method appeals may wish to apply it to frames with hinges at the ends, it does not seem particularly well adapted to such usage. A two-hinged frame is statically indeterminate only to the first degree, and very simple methods are available for such a problem.

In summary, it is believed that this method offers certain advantages in cases where end movements are complex. These advantages are emphasized for engineers who prefer graphical constructions. Those who have mastered the method may want to use it for more general loadings, even though it seems to hold no great advantage in that respect over the column analogy method.

Corrections for *Transactions:* In November, 1947, *Proceedings,* on page 1391, line 12, add "graphically" as the last word of the paragraph; on page 1392, line 1, add "planar" between the words, "any" and "structure," and, line 2, at the end of the paragraph, add the sentence: "The examples given are graphical illustrations of the application of the method and are not exact solutions"; on page 1402, Fig. 11, change "$Z_{oa} = 33.8''''$" to "$Z_{oa} = 38.8''''$"; on page 1405, change Eq. 24 to read: "$\Delta_z = \Delta_{zR} + \Delta_{zT} = \dfrac{F_o Z^2{}_o L_o}{E I} + \dfrac{F_o I_{zo}}{EI}$"; and, on page 1406, change Eq. 25*a* to read: "$\Delta_{zR} = \dfrac{Z^2{}_o L_o}{I_{zo} Z^2{}_o L_o} \times \Delta_z$," and change Eq. 25*b* to read: "$\Delta_{zT} = \dfrac{I_{zo}}{I_{zo} Z^2{}_o L_o} \times \Delta_z$."

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DISCUSSIONS

PROBLEMS AND CONTROL OF DECENTRALIZATION IN URBAN AREAS A SYMPOSIUM

Discussion

BY DONALD M. BAKER

DONALD M. BAKER,[4] M. ASCE.—The United States, since its founding in 1787, steadily has shown a continued trend toward greater urbanization (see Table 1).

By 1920 slightly more than one half of the population of the United States lived in cities, one quarter of the people centering in cities with a population of more than 100,000. Although the increase in proportion of urban population between 1930 and 1940 was negligible—from 56.2% to 56.5%—and undoubtedly was due to the "back to the farm" movement during the depression of that period, the 1950 census should show a continuation of the major trend as a result of activity in war industry in urban areas.

TABLE 1.—TREND OF URBANIZATION OF POPULATION IN THE UNITED STATES

Census year	Percentage of Population		Cities with Populations in Excess of 100,000	
	Urban	Rural	No.	Percentage of national population
1790.....	5.1	94.9	....	....
1860.....	19.8	80.2	....	....
1890.....	35.1	64.9	28	15.4
1920.....	51.2	48.8	68	26.0
1940.....	56.5	43.5	92	29.8

These data do not present a complete picture either as to increasing urbanization or as to decentralization of urban areas. In 1930, 44.7% of the national

NOTE.—This Symposium was published in November, 1947, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: March, 1948, by William T. Hooper.

[4] Ruscardon Engrs., Los Angeles, Calif.

population lived in metropolitan districts each of which had a population within its central city or cities and satellite area in excess of 100,000; in 1940 this proportion had increased to 44.8%. Between 1920 and 1930 the population of these districts increased 28.3%, as against a national rate of increase of 16.1%. Population in the central cities of these districts increased 22.3%, whereas that in satellite areas increased 44.0%. Between 1930 and 1940, the increase in national population was 7.3%; in these same metropolitan districts, 7.7%; in the central cities of these districts, 4.7%; and in their satellite areas, 14.4%.

The continued trend has been for people to move from rural to urban areas, but for the greatest population increase to occur in peripheral sections of these areas. Whether the added population located in the peripheral areas because space was available there, or whether the older residents of these areas moved outward, creating a vacuum in the central area which attracted the new population, is primarily a problem for sociologists. Prior to about 1910 much of the increase in urban living undoubtedly came from foreign immigration, but since that date a large proportion of it has come from rural areas. Mechanization of farming operations has no doubt been an important factor, and increase in industrialization in urban areas has provided a means of livelihood for new inhabitants.

Three distinct stages have occurred in the development of the American city, due to increasing population and geographical size. These have, to a large extent, been governed by the then existing modes of transportation. During the first part of the nineteenth century, urban populations were small and were generally grouped around a central district occupied by offices, stores, and shops. Such industry as existed was usually located close to the central district. The developed area was small in extent and population densities were low because daily travel distances were not great. Travel was by private carriage, on horseback, or on foot—at speeds ranging from 3 miles per hr to 6 miles per hr. Public transportation was not required. When it did become necessary, it was provided by horse-drawn omnibuses, which had travel speeds of from 4 miles per hr to 6 miles per hr. These travel speeds limited residential development to a distance of about five miles from the central district.

In the 1870's horse-drawn cars superseded the omnibus, with slightly greater speeds of travel, and allowed settlements to spread out somewhat.* Settled areas still remained compact with only slightly greater population densities. People in the higher income brackets moved outward to peripheral areas, and those in the lower brackets moved into their vacated residences.

The second stage commenced around 1890. Cable cars superseded horsecars, and these were soon superseded by electric streetcars having travel speeds of from two to three times those of the outmoded vehicles. Use of electric cars allowed the population to spread out more, but densities increased. Extensions of streetcar lines into undeveloped areas usually were made only after the areas served were fully developed. This had a tendency to increase population densities and to cause development to remain in a compact form. Daily movements of people were still along radial lines, between homes and the central or industrial district. In the larger cities, because travel between

homes and stores in the central district required too much time, stores supplying necessity shoppers (food, drugs, hardware, and some service trades) sprang up along streetcar lines, especially near transfer points.

About this time, commuter trains commenced to make their appearance on steam railroad lines, and satellite residential communities sprang up, separated from the central city by vacant or farm land. These were inhabitated largely by people in the upper income brackets, who could afford the cost of daily transportation, and those serving these people. Retail stores to serve the suburbanites developed along these lines, but most of the trade and business was still carried on in the central districts. During the decade prior to World War I, electric interurban lines were constructed in a number of communities, and New York (N. Y.), because of its peculiar size and shape and its surrounding water area, developed off-surface rapid transit.

Following World War I, construction and operating costs on rail transit lines increased, while the traveling public still insisted on a prewar fare. As a result, operation of transit facilities became less and less profitable, and in many cases definitely unprofitable. Equipment and service deteriorated and new capital for extensions and improvements was difficult or impossible to secure. Extensions and improvements, therefore, were not made.

Prior to 1920 communities had grown in a more or less hit-or-miss fashion. Such through streets as existed were laid out along roads that connected the central district and the outlying suburban or farming centers, or had followed property boundaries. Sometimes these were made wider than interior streets, sometimes not. Streetcars and horse-drawn carriages did not cause traffic congestion, even on narrow streets, and street jogs caused no inconvenience in travel in the existing slow speed of movement.

Following 1920, however, the private automobile came into common use. The national rate of motor vehicle registration increased from 35 in 1,000 (1916) to 87 in 1,000 (1920), and to 215 in 1,000 (1930). People found that they could live half a mile from a streetcar line if they owned and used an automobile, and still get around. Urban population commenced to decentralize at a rapid rate, and developed urban areas commenced to assume a far less compact shape.

At this time also city planning began to be recognized. Planning commissions were established in many communities. Land was subdivided in peripheral areas in a more intelligent manner. Attempts were made to provide arterial highways of proper width and in suitable locations in newly developed areas. Street jogs were eliminated, and street widths were given more careful attention. Population densities were lowered, as a result of zoning in newer areas. These zoning ordinances controlled land use, and segregated various types to more suitable locations. In the studies made to establish these ordinances, basic data relative to land use were obtained, as was also knowledge regarding city patterns and structure. As a result, the newer sections of urban areas developed after 1920 did not encounter all the problems found in the older sections.

Even by 1920, however, there had been little decentralization of central districts. Stores serving luxury and selective shoppers were still located there, but during the 1920–1930 decade the increase in motor vehicle ownership gave

rise to problems of traffic congestion, particularly on approach streets to central districts. Heavy motor vehicle traffic slowed down the speed of streetcars, and more and more riders began to use their own automobiles. The habit of riding on transit facilities dropped, net revenues fell, and a vicious spiral commenced.

Extensive programs of street openings and street widenings were carried out in the 1920–1930 decade, but as fast as new arterial highways were constructed more automobiles were attracted to them; little or no relief from traffic congestion resulted. By 1930 most stores serving necessity shoppers had moved to outlying areas, although those serving luxury and selective shoppers still remained at locations in the central district.

Rail transit companies entered the 1930 depression with low riding habits, low net income, and low morale. Such extensions of lines as were made were bus lines, which did not require heavy investment in rails, rights of way, or power equipment, and which had far greater flexibility in routing. During the 1930 decade the American Transit Association, through its committees, developed the design of what was known as the President's Conference Car, a streamlined electric transit vehicle, having far easier and more comfortable riding qualities, being lighter in weight, and having greater acceleration—thus allowing an increase in scheduled speeds. Its use on a few lines spurred patronage somewhat, but riding habit and net income showed little or no over-all improvement. Motor vehicle registration increased to 250 per 1,000 population in 1940, and use of motor vehicles as a mode of mass transportation increased at a much greater rate.

Between 1930 and 1940 the rate of population increase in urban communities dropped to about that of the national increase. Depressed business conditions discouraged the construction of buildings in central districts and the expansion of retail trade to outlying areas. Such public improvements as were carried on were primarily to provide employment; those which sought to alleviate traffic congestion and improve circulation in urban areas were relatively few in number. Much constructive work was done, however, in the collection of basic data pertaining to traffic, circulation, and land use, and in the preparation of plans to remedy the problems thereof.

World War II more or less "froze" conditions to the status of the 1930–1940 decade, but it created a prosperous period for public carriers, which lasted throughout the war and for a year or two following its termination.

The elimination of gasoline rationing and the increase in motor vehicle production quickly caused traffic congestion of a most serious character to occur, and also caused the parking problem in central districts to become extremely acute. New housing, located for the most part in peripheral areas where land had been developed in a more intelligent manner, served to decentralize population further. Because of prosperous business conditions many retail stores in central districts gave serious thought to the problem and actually moved to outlying sections or opened large branches there, with large parking areas for their customers.

Parking in central districts has become an even more serious problem than traffic congestion on streets approaching these districts. Many cities now

require that all new buildings constructed in their limits provide automobile parking space. Los Angeles (Calif.) has a provision in its zoning ordinance which requires that parking space for one automobile be furnished for every 1,000 sq ft of gross floor area in a new building, such space being located at the building site or within 1,500 ft of it. This means that about 200 sq ft of parking space must be provided for every 1,000 sq ft of gross floor area. Such a provision greatly adds to construction cost in central districts. In the case of retail stores and many office buildings it may make new construction in central districts uneconomical, or at least retard it greatly, forcing new development to the outlying sections where ground-level parking on cheaper land can be had.

All the forces which tend to mold the pattern of development of urban areas would appear to encourage future decentralization of population and retail trade. This does not mean that central districts will eventually disappear, but that the future activities of these districts will be of a different character. Business operations, requiring people to work in close contact with large numbers of other people, and small, light industries, will ultimately predominate. Such districts will not grow at a rate comparable to that of the urban areas they serve; many people will still have to work there, and travel daily between the central district and their residences. It will also take considerable time for this transition in activities to occur. In the meantime, people will continue to move to peripheral areas—not to save taxes so much as to be able to live under less congested conditions—to own, keep, and use an automobile, or even two; to have a yard for their children and a garden for themselves; and to be able to enjoy the other amenities of life, which go with higher living standards and which cannot be achieved through living in apartments or tenements.

Expressways leading to central districts, and to other sections of urban areas, will be necessary. Whether transit companies will be able to operate profitably through increased fares, increased riding habit due to improvements in equipment, and faster schedules must remain to be seen. They will have an opportunity to do this in some localities where the scarcity and the high cost of parking may force more people to ride on transit lines.

Whatever decentralization of central districts and of population occurs in the future, most of the problems discussed in the Symposium will remain, and their satisfactory solution will involve large sums of money. The average American citizen, however, is like a man with poor teeth. He will readily admit that from the standpoint of economy and health it is advisable to "brush his teeth twice a day and see his dentist twice a year," yet he continues to stay away from his dentist until he has an excruciating toothache, and finally needs a complete set of dentures long before he should have them. Radio, undoubtedly, has done much to change the public's attitude toward dental care, but it will require much more than the studies and reports of social workers and city planners to convince the general public that the cost of playgrounds is more than repaid in better community health and in reduction in crime and juvenile delinquency, or that the cost of expressways is offset by reduction in automobile accidents, operating expenses, and travel time.

The person who invented the gasoline tax had the right idea; this tax is painless, it is collected in small installments, and it immediately produces tangible returns in miles of hard-surface highways. Its present rate of receipt, however, is inadequate to finance construction of the needed expressways in time for them to be of value to this generation. With present low interest rates, this tax, or part of it, say, that which is collected in urban areas, could be utilized to pay principal and interest on bonds that could be issued in amounts sufficient to pay for the construction of substantial mileages of expressways at an early date. This would do much to solve some of the most pressing problems of urban areas.

The greatest task now facing political scientists—including social workers, planners, and engineers—who are interested in the problems facing urban areas, is not that of investigating and stating the existing problems and suggesting solutions for them, as much as it is that of developing adequate and painless methods of financing these solutions, and of selling those who must pay for these solutions on the fact that money spent for these purposes is a sound investment.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

EXPERIENCES WITH PREDETERMINING PILE LENGTHS

Discussion

BY HAMILTON GRAY, JACOB FELD, AND GEORGE F. SOWERS

HAMILTON GRAY,[4] Assoc. M. ASCE.—Foundation engineers have long known from experience that the supporting power of piles embedded in cohesive soils cannot be predicted with consistent reliability on the basis of observations made during driving operations. Hence, the use of dynamic pile formulas, when resistance to penetration is derived from cohesive soils, properly meets with scant favor at the present time. Static field loading tests appear to furnish far more reliable information in such cases and, in spite of their expense, are unquestionably justified whenever the cost of the foundation exceeds a modest sum.

Prior to the performance of such tests, a relatively inexpensive attempt may be made to anticipate the test results. Logically, such an attempt involves the static resistance, furnished by the surrounding soil, to relative displacement of the embedded pile, and therefore requires a knowledge of the pertinent physical properties of the surrounding earth. This problem is discussed briefly elsewhere,[5] where the shortcomings of the static approach are described. These shortcomings appear fully applicable to the method described by Mr. Moore, whose analysis is, however, more acceptable in that it requires the determination of the physical properties of the soils actually underlying the building site, rather than the use of "handbook" or average values. Nevertheless, the uncertainties involved should not be minimized. It is also generally accepted, by virtue of experience, as well as by theoretical indications, that the behavior of a group of loaded piles driven into any but the most compact materials will be quantitatively unlike the behavior of a single loaded pile.

The analysis of the behavior of a loaded pile embedded in cohesive soils would appear to require two main considerations: First, the displacement or slipping of the pile relative to the surrounding earth, in which category must

NOTE.—This paper by William W. Moore was published in November, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1948, by A. W. Earl.

[4] Prof., of Civ. Eng., Univ. of Maine, Orono, Me.

[5] "Pile Foundations and Pile Structures," *Manual of Engineering Practice No. 27*, ASCE, 1946, pp. 71–72.

also be classed the slipping of a roughly cylindrical mass of stiffened soil, which may adhere to, and move with, the pile, relative to the weaker surrounding soil; second, the displacement, due to volume change or consolidation, induced by the compressive stresses created in the soil by the loaded piles. The analysis of these two modes of behavior is fraught with uncertainty.

When a pile is driven into cohesive soils, these are violently displaced with a consequent alteration of the grain structure or configuration. Many soils are amazingly weakened by this distortion, the ratio between the unconfined compressive strengths of "undisturbed" and "molded" samples of some clays reaching values well in excess of 20. Temporarily, therefore, the displaced material immediately surrounding the driven piles may be considered weaker than the original deposit, although clays appear to exist which present exceptions to this statement. In time, however, consolidation of this reworked material occurs, as a result of which it generally acquires a greater strength than the remainder of the stratum. Consequently, it is to be expected that the slipping tendency of a pile is greatest immediately after driving, and diminishes with the passage of time. The rate of increase of resistance depends on the drainage characteristics of the soil. Thus, cohesionless soils possess such large permeabilities that the gain in resistance is usually so rapid that for practical purposes it may be considered instantaneous. The time increase in driving resistance offered by a pile embedded in cohesive soil, and the mass of cohesive earth that often adheres to piles pulled after driving, appear to substantiate the foregoing concept.

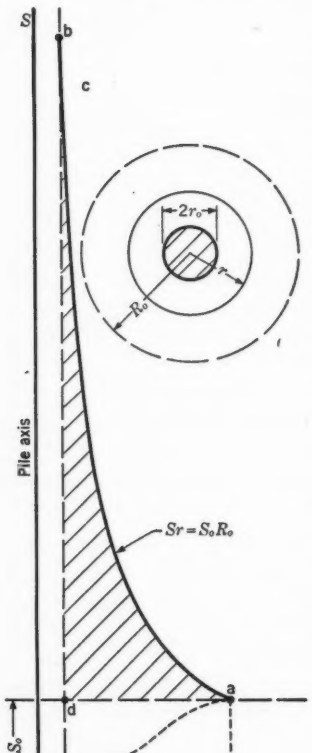

FIG. 19—POSSIBLE VARIATION IN STRENGTH OF SOIL SURROUNDING A PILE

Behavior of the earth immediately surrounding a pile may be visualized by a consideration of Fig. 19, which pertains to an isolated pile driven into a clay deposit. Here, the behavior of the soil after driving of the pile is represented graphically. Assuming for simplicity that the clay stratum penetrated by the pile is homogeneous, its shearing strength at a given depth prior to the driving of the pile may be represented by S_o, the value of which is essentially a function of the void ratio, or water content of the soil, and of the particular soil type. As the pile is driven, the molding effort is felt appreciably within a radial distance denoted by R_o. In all probability, the degree of soil disturbance and the degree of structural weakening increase as r diminishes. Immediately after driving, the strength

may vary according to some such relation as indicated by the curve a-a. However, by virtue of subsequent consolidation of the molded material, the strength of the soil within the distance R_o increases until, upon pulling the pile, the soil fails along some quasi-cylindrical surface not larger than R_o. If F denotes the pulling force per foot of pile length, the average shear stress along a cylindrical surface of radius, r, must be

$$S = \frac{F}{2\pi r} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

Should the slipping actually occur along the cylindrical surface with radius R_o, the shear stress intensity on smaller concentric cylindrical surfaces would be represented by the ordinates of curve ab in Fig. 19. The strength of the soil would be represented by a curve with ordinates greater than those of ab—for example, ac. Should slipping occur on a surface with diameter less than $2R_o$, the strength of the soil must be less than the ordinates of curve ab at some value of $r < R_o$. In any event, the strength when $r_o < r < R_o$ must, if soil adheres to a pulled pile, lie within the limits of the crosshatched zone, abd, in Fig. 19. It must not be assumed that slipping would actually occur along a true cylindrical surface, the use of such a surface being merely for convenience in analysis.

On the other hand, the strength of a soil which is not sensitive to molding would remain essentially at the value S_o. Since the author apparently used the value S_o as a conservative value, it would seem that the actual factor of safety against slipping would vary from locality to locality—depending on the sensitivity of the various clays to disturbance. The same considerations would apply to groups of closely spaced piles.

Having determined by conservative methods that the piles are safe from slipping, the question of subsidence of the supporting medium must be faced. The manner in which the piles transmit their load to the soil, and the stress distribution around and beneath the piles, are matters about which no quantitative information appears to exist. Therefore, only crude guesses can be made as to the expected subsidence. In the examples cited by the author, no consequential subsidence developed; this apparently was due to the stiffness of the firm clays into which the lower ends of the piles penetrated, a consistency acquired perhaps from some large prehistoric overburden. It is interesting to speculate on the possibility that a clay firm enough to carry a loaded pile without slipping can also carry its load without undergoing undue volume change. Certainly the observations and predictions cited in this paper indicate this.

The author states that "yield point shear strengths" were determined. This term requires definition since in testing a structural material "yield point" is defined on the basis of the slope of the stress-strain diagram, and "yield strength," by the shape of the stress-strain diagram beyond the proportional limit. Presumably, the tests were performed at high rates of stress increase to avoid consolidation during the testing.

No novel theory appears involved in the method, and engineers will appreciate the logical nature of this approach to the problem—combining as it does ordinary statics and the shearing properties of the surrounding soil. However,

it would be well to bear in mind (*a*) the uncertainties involved in estimating the actual shearing strength of the soil about the pile, as well as (*b*) the assumptions regarding the mode of failure of the pile. General acceptance should be preceded by satisfactory experience with the method in various types of clays, and, to this end, added factual data on the effect of disturbance, shearing properties, and Atterberg limits of the various soils described in the paper would be valuable.

JACOB FELD,[6] M. ASCE.—The director of the Patent Policy Survey of the National Research Council, Archie M. Palmer,[7] in a report favorably inclined to the patenting of scientific and technical discoveries, recently stated:

> "Discoveries or inventions possessing commercial application that are merely published and are thus made available to everybody equally are seldom adopted."

Karl T. Compton,[8] President of the Massachusetts Institute of Technology at Cambridge, is also quoted, from an annual report, as being in agreement with such advice:

> "Responsibility does not always end with the mere publication of patentable scientific discovery or invention; the public benefits derivable from the patent laws and contemplated by the framers of those laws should not be lost through a failure to solicit patent protection."

Even more to the point, Elihu Thompson[9] in 1920 advised in an address before graduating students at the Massachusetts Institute of Technology:

> "Publish an invention freely, and it will almost surely die from lack of interest in its development. It will not be developed and the world will not be benefited. Patent it, and if valuable, it will be taken up and developed into a business."

Perhaps it was such thoughts that induced the author and his associates to apply for and accept patent rights for the method described in this paper, even if, as the author states in the "Conclusion":

> "The methods of analysis employed for predetermining pile lengths are in a formative stage, and it is hoped they will be improved as knowledge increases."

These quotations agree in assuming that the discovery to be patented is correct as well as commercially applicable. No doubt the construction industry would welcome a new, correct method of predetermining pile lengths and would gladly pay for the privilege of profiting by the savings through such an invention. However, the method described in this paper is neither new nor correct. In 1932 the writer outlined the method described in this paper and also showed what uncertainties and errors were involved.[10] However, he makes no claim

[6] Cons. Engr., New York, N. Y.
[7] "Patents and University Research," by A. M. Palmer, *Scientific Monthly*, February, 1948, p. 149.
[8] *Technology Review*, December, 1932, p. 101.
[9] *Electrical World*, Vol. 75, 1920, p. 1505.
[10] "Point Resistance of Piles Analyzed," by Jacob Feld, *Civil Engineering*, December, 1932, p. 779.

to having discovered a new method at that time, for exactly a century earlier many of the concepts were published by M. Moreau.[11]

There are a number of basic errors in the method described in the paper:

1. Eq. 1 assumes that the lateral pressure against the face of a pile at any depth is the sum of two items. The first is the same as the "equivalent liquid pressure" found in many discussions of lateral earth pressure and is given as a constant, K, times the total weight of the overlying soil. It has been shown that the lateral pressure against a vertical face of narrow dimension, such as a pit or a pile, is theoretically not a linear function of the depth.[12] Actually, at certain depths, there is no lateral pressure, depending on the ratio of width to depth and on the soil characteristics. This theoretical consideration is corroborated by the well-known experience with pressures to be resisted in sheathed pits[13] and by the fact that cored holes and dug wells are possible in most soils. Even in the general case of earth pressure against unlimited wall faces, the factor K is not a constant unless the soil is uniform, and even in that case there is some doubt as to whether depth is not an influencing variable.

2. The last term of Eq. 1 is a function of the shearing strength of the soil. The yield point bearing of the soil in Table 2, Col. 7, seems to be derived from the assumption that, since the dilatancy of elastic bodies undergoing elastic strain is equal to the sum of strains along the three rectangular axes, the bearing strength is equal to three times the linear shearing strength. This may be approximately true for isotropic bodies in which the elastic limit is not exceeded, if the higher powers of the infinitesimal strain values are disregarded, but such a condition does not describe the shoving away of the soil by the introduction of a pile.

3. The assumption that frictional resistance of soil along the face of a moving body can be described in terms of the laws of static friction, as first postulated by Coulomb, is not consistent with the action of pipes jacked through soil, of caissons sunk vertically, of shield driven tunnels, or of piles undergoing loading and pull-up tests. The frictional resistance in all these cases can be explained only by the laws of viscous flow, with proper consideration for adhesion and tackiness.[14,15]

4. The average friction along the surface of the piles cannot be based on a coefficient of friction greater than that of the soil itself, as is shown in Fig. 2 by the values of α being greater than those of ϕ. Actually, in Table 2, Col. 6, values of frictional resistances on wood are smaller than the soil shearing strength at the corresponding depths in Col. 5. It can be easily seen that, where the frictional resistance exceeds the soil shearing strength, failure will occur in the soil immediately outside the contact surface of the pile.

5. The method of evaluating the total pile capacity, shown in Table 3, is an arithmetic summation of resistances computed in the various soil layers.

[11] "Notice sur une Nouvelle Manière de Fonder en Mauvais Terrain," by M. Moreau, *Memorial de l'Officier du Génie*, No. 11, Paris, 1832, pp. 11–40.

[12] *Transactions*, ASCE, Vol. 86, 1923, p. 1579.

[13] "Precast Concrete Sheathing," by Jacob Feld, *Journal*, A. C. I., Vol. 16, 1945, p. 441.

[14] "Soil Resistance to Moving Pipes and Shafts," by Jacob Feld, to be presented at the International Soil Mechanics Conference, Rotterdam, Holland, 1948.

[15] "Jacked in Place Pipe Drainage," by Jacob Feld, presented at the annual meeting of the Highway Research Board, National Research Council, Washington, D. C., 1947.

This method is exactly equivalent to assuming that the strength of a laminated beam consisting of nonequal planks laid flat on each other is the summation of the individual strength of the planks. Unless the strains are equal at the resistances considered, such an arithmetical summation has no meaning and is a serious error.

The comparison of pile capacity determinations listed in Table 8 indicates that the *Engineering News* formula used does not apply to the driving conditions, especially when the heavy Vulcan No. 1 and Vulcan No. 0 hammers were employed. The author does not state which form of the formula was used, but it is well established that the original formula, as well as modifications introduced for applying steam hammer impact, does not indicate true bearing values of piles when such large hammers are used.

GEORGE F. SOWERS,[16] JUN. ASCE.—Methods such as advocated by the author in this significant contribution to foundation engineering not only aid in predetermining pile lengths, but also point out the significant factors that make up the capacity of a pile. The usual methods of pile "design" to support a given load can be summarized under the headings: (1) Guess or estimate based on experience; (2) Driving of piles to required resistance to penetration as determined by one of nearly a hundred formulas; and (3) Driving of several piles to different depths and load testing each to determine those which are satisfactory.

The frequent failures of pile foundations to meet expectations when designed on the basis of experience alone, or on the basis of formulas, indicate the shortcomings of the first two methods. The test pile method is now required on most major jobs to insure adequate foundation capacity, but it furnishes information on only the characteristics of similar piles driven in the same manner at the same location as the test piles. The test results tell nothing about how pile capacity might vary with different piles at different locations. The rational method of pile analysis proposed by the author not only gives the capacity of a pile, but also indicates how that capacity might change with variations in length or cross section, and, if adequate soil borings were made, how capacity might change throughout the building site.

The writer had available boring records, pile test data, and some meager information on the soil characteristics at two sites in the Cuyahoga River valley at Cleveland, Ohio. The borings and pile tests were made under the direction of G. B. Sowers, M. ASCE, and detailed descriptions of the foundation conditions encountered are available elsewhere.[17,18] The Cuyahoga River valley in Cleveland lies to one side of a deep preglacial valley filled with deposits of fine sand, silt, and clay. The two sites are about 1 mile apart, and although similar types of soils are encountered at each site, the soil profiles are different. At the first site, the Republic Steel plant built in 1943, the soil is of medium

[16] Asst. Prof., Civ. Eng., Georgia School of Technology, Atlanta, Ga.

[17] "Through 130 ft of Plastic Clay to Rock," by George B. Sowers, *Engineering News-Record*, November 18, 1943, p. 107.

[18] "The Cuyahoga River Improvement 1938–1942," by G. B. Sowers, thesis presented to the Case School of Applied Science at Cleveland, Ohio, in December, 1942, in partial fulfilment of the requirements for the degree of Civil Engineer.

consistency, gray silty clay of medium plasticity with thin seams of silt and occasional thick strata of stiff silty clay (Fig. 20(*a*)). At the turning basin, built in 1941 as part of the Cuyahoga River Improvement, fine sand overlies the clay deposits to depths of 50 ft (Fig. 21(*a*)).

Soil tests for shear and for friction on steel were made in the laboratory at Case Institute of Technology (formerly Case School of Applied Science) in Cleveland, on samples from the turning basin site.[19] Unfortunately only index classification tests were made on the soils on the Republic site;[18] however, the shear and friction characteristics were estimated from the penetration resistance and by comparison with the other site. Although the soil data are inadequate, it is believed that the strength and friction profiles in Figs. 20 and 21 are a fair representation of the true soil characteristics.

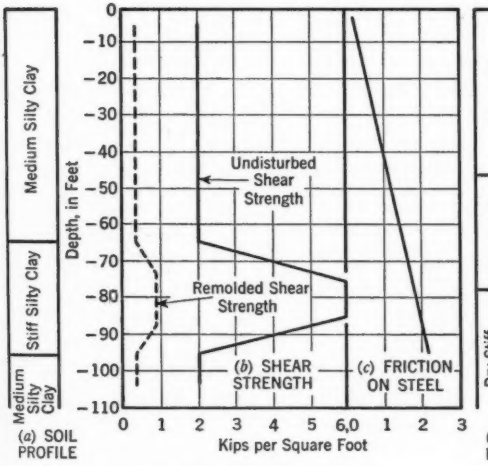

FIG. 20.—SOIL DATA FROM REPUBLIC STEEL SITE

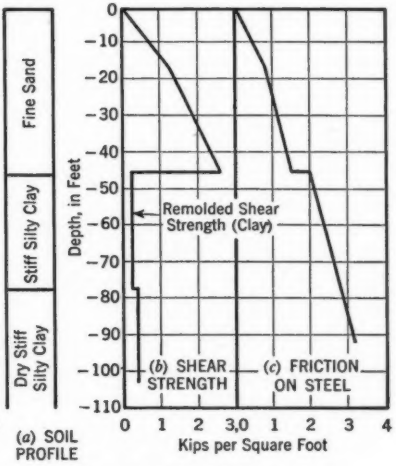

FIG. 21.—SOIL DATA FROM TURNING BASIN SITE

Nearly one hundred pile tests in compression were made at the Republic site and five tests in both tension and compression at the other. Four of the Republic piles were analyzed for bearing as described by the author. The first computation used undisturbed shear characteristics of the soil; a second computation was made using the remolded shear characteristics. Two piles at the turning basin site were analyzed using the remolded shear strength of the soil. Lateral earth pressure in both cases was assumed to be equal to the vertical pressure.

The results of computing pile capacities at the Republic site from the undisturbed soil characteristics indicate a serious error, for computed capacities are more than double the test capacities (Table 9). When the remolded shear strength was used, the computed capacities were comparable to the pile test results, the greatest error being about 14%. This is excellent agreement con-

[19] "A Comparison of a Laboratory Determination of the Tension Value of Steel Piling with Field Tests," by E. Schmidt, thesis presented to the Case School of Applied Science at Cleveland, Ohio, in June, 1941, in partial fulfilment of the requirements for the degree of Bachelor of Science.

sidering the inadequacy of the soil information. At the turning basin site the computed capacities (based on the remolded shear strength of the clay strata) were higher than the test pile results, but again the agreement was well within

TABLE 9.—SUMMARY OF RESULTS AT REPUBLIC STEEL SITE (CLEVELAND, OHIO)

Test pile No.	Size[a]	Length (ft)	Test load (tons)[b]	Computed Load (Tons)	
				Undisturbed	Remolded
(1)	(2)	(3)	(4)	(5)	(6)
4	10¾ in.	78	45	104	47
5	10¾ in.	81	50	109	52
6	10¾ in.	83	45	114	50
7	16 in.	89	95	172	89
8	16 in.	69	70	144	60

[a] Steel pipe, outside diameter. [b] Compression.

the accuracy of the soil information (Table 10). In this case the best agreement was for the steel WF pile (assuming the soil between the flanges to act as part of the pile).

The results of these computations led the writer to check in detail the assumptions of the author's method in order to suggest improvements in the procedure. As the author states, the method is in the formative stage and further analysis is needed.

TABLE 10.—SUMMARY OF RESULTS AT TURNING BASIN SITE

Test pile No.	Type	Length (ft)	Test Load (Tons)		Computed Load (Tons)	
			Tension	Compression	Tension	Compression
(1)	(2)	(3)	(4)	(5)	(6)	(7)
5	10 WF 54	76	90	90	97	98
6	12¾-in. steel pipe[a]	78	85	100	91	100

[a] Outside diameter.

Experienced construction men recognize that entirely different characteristics are exhibited by silts, sand, and gravel, on the one hand, and clay soils on the other. Any method for analysis of piles should recognize this difference when determining lateral pressure along the pile, friction on the pile surface, and soil shear strength.

In granular soils, test borings and laboratory investigations furnish information to determine the vertical pressure in the soil deposit, $\Sigma\gamma h$; void ratio and relative density; the angle of internal friction of the soil, ϕ; and the coefficient of friction of the soil on the pile, tan α. The angle of internal friction of granular soils varies with relative density. Pile driving tends to compact such soils to their densest condition, and, therefore, ϕ, for the densest state of granular

soils, should be used. The greatest difficulty lies in computing the lateral soil pressure. In natural sand and gravel deposits the lateral pressure P_l ranges from 0.4 times to 0.6 times the vertical pressure P_v. If the author's formula for friction on the pile (Eq. 6), which he calls modified friction in order to account for increase in lateral pressure due to pile displacement, is rearranged,

$$F_m = (P_v + \pi S) \tan \alpha \ldots\ldots\ldots\ldots\ldots\ldots (9)$$

or, in other words,

$$P_l = P_v + \pi S \ldots\ldots\ldots\ldots\ldots\ldots (10)$$

Substituting in Eq. 10 the expression for shear strength of granular soils,

$$S = P \tan \phi \ldots\ldots\ldots\ldots\ldots\ldots (11)$$

Where $\tan \phi$ ranges in value from 0.6 to 1.0 for typical sands and gravels,

$$P_l = 3 \text{ to } 4\, P_v \ldots\ldots\ldots\ldots\ldots\ldots (12)$$

This is an increase in P_l of about 600% over that of the natural deposit. The writer can see no logical justification for this tremendous increase in pressure. In loose sands the soil structure breaks down with the driving, and in pass ing from the loose to the dense state the pressure may increase only slightly. Application of the "modified friction equation" (Eq. 6) to the turning basin piles gives results far too high. In dense sands such great difficulty is experienced in driving that jetting is used to sink the piles, and the resulting lateral pressure is probably close to that of the natural deposit. At any rate, much study is needed on this question.

The end bearing of a pile in sand or gravel is probably too low when computed by Eq. 7. The basis of Eq. 7 is the expression derived by L. Prandtl and quoted by A. Nádai:[20]

$$q = \pi S \ldots\ldots\ldots\ldots\ldots\ldots (13)$$

in which q is the bearing capacity per square foot. Eq. 13, however, was based on the assumptions of: (*a*) Cohesive, weightless soil; (*b*) Continuous footing; (*c*) frictionless base of footing; (*d*) footing resting at the surface of the soil; and (*e*) failure occurring as soon as plastic equilibrium is established at any point.

None of these conditions are met when the formula is used to estimate end bearing of piles in sand. Probably the worst discrepancy between theory and reality results from assumptions (*a*) and (*d*). K. Terzaghi,[21] Hon. M. ASCE, gives the unit bearing capacity of a round footing on a cohesionless soil to be approximately

$$q = \gamma D_f N'_q + 0.6\, \gamma R N'_\gamma \ldots\ldots\ldots\ldots\ldots\ldots (14)$$

in which D_f is the depth of footing below the ground surface; R is the pile radius; and N'_q and N'_γ are factors for local shear failure that depend on the angle of internal friction, ϕ. For an 80-ft pile with a point diameter of 1 ft driven in an average saturated sand or gravel ($\phi = 35°$ and $\gamma_s = 60$ lb per cu ft), the end bearing capacity computed by Eq. 13 would be 67,000 lb per sq ft, as compared

[20] "Plasticity," by A. Nádai, McGraw-Hill Book Co., Inc., New York, N. Y., 1931.
[21] "Theoretical Soil Mechanics," by K. Terzaghi, John Wiley & Sons, Inc., New York, N. Y., 1943.

with 11,300 lb per sq ft when computed by the author's formulas. Although Eq. 13 applies to footings where D_f is small compared to R, it illustrates the possible magnitude of error in applying Eq. 7 to cohesionless soils.

In clay soils the phenomena that result in pile bearing capacity are more complex. The vertical pressure and void ratio are determined by laboratory tests. In natural deposits the clay does not necessarily follow the commonly accepted law of increase in shear strength with pressure expressed by Eq. 4, but may actually decrease in shear strength with depth. This necessitates shear tests (unconfined compression tests are adequate and simple) on samples taken throughout the deposit. A chart showing shear versus depth can be made, such as Figs. 20(*b*) or 21(*b*). In addition, the variation of shear strength with remolding and subsequent consolidation must be considered.

There is some question in the writer's mind whether true friction exists at all between a clay soil and a pile. The few clay friction tests he has conducted or observed indicate that the resistance to sliding along a steel surface (be it friction or adhesion) is considerably more than the remolded shear strength of the clay. This results in a smear of clay remaining on the steel surface as movement takes place. Observation of piles pulled from clay with such smears of clay adhering to the surface indicates that the surface phenomenon of sliding resistance is not as important as remolded shear, but more study is needed to clarify this point.

A most important fact brought out by the author, and in another manner by the writer's analysis, is that pile bearing in clay varies with time. The author ably discusses the changing effect of soft strata along the upper sections of a pile (from support of the pile to drag on the pile). The change in the shear characteristics of clay and its effect on pile support, however, are not discussed.

A. Casagrande,[22] M. ASCE, has pointed out that, as a pile penetrates clay, a zone of shear failure and plastic flow is created by the point. This remolded zone surrounds the pile throughout its length and has an unknown thickness of from perhaps a few inches up to one and one-half times the pile diameter. The surface of the pile is therefore supported by remolded clay, but the stressed zone at the point reaches into the undisturbed clay. The computations summarized in Tables 9 and 10 indicate that this was the case for the glacial clays at Cleveland, where the remolded shear strength is about 15% of the undisturbed shear values.

As time progresses the remolded soils surrounding the pile consolidate because of the breakdown of their structure, and, at the same time, some of the disturbed colloidal gels tend to resolidify. This zone, then, gains in shear strength with time, and in some soils may even exceed the strength of the original undisturbed soil. What happens to the friction or adhesion along the pile surface is unknown, but it probably increases too. In other words, the soft disturbed zone eventually becomes a hard zone, attached to the pile more or less securely. Part of this increase in strength of the disturbed zone occurs rapidly, as is illustrated by the increase in driving resistance after a short rest has occurred. How long it will take for complete consolidation and gain in

[22] "The Structure of Clay and Its Importance in Foundation Engineering," by A. Casagrande, in "Contributions to Soil Mechanics—1925–1940," Boston Soc. of Civ. Engrs., Boston, Mass., 1941, p. 91.

strength to take place depends largely on the permeability, void ratio, and thickness of the soil. Old piles pulled from clay often bring with them a 1-in. coating or a 2-in. coating of hardened clay.

Another factor not discussed by the author is the relative strain required to mobilize the full shear or friction resistance of each stratum. Slipping or full failure in a hard stratum may occur before much resistance is developed in a soft stratum; and, if the shear or friction resistance of the soil after failure is less than that at failure, the pile will fail progressively at a lower loading than the computations might indicate. However, this is probably of minor importance and could be neglected inasmuch as the softer layers contribute only a small part of the total pile capacity.

Of course it must be recognized that two questions are left unanswered by this analysis:

1. How much load will a pile group carry?
2. How much settlement will occur if the piles can carry the load without slipping into the ground?

The author's analysis, by increasing our knowledge of the factors that contribute to the capacity of a single pile, opens the way toward rational analysis of pile groups and pile settlement.

Probably many so-called practical engineers will scoff at the detail involved in a rational analysis of a pile foundation. Engineers who go to great lengths to design a building superstructure to the last rivet often "design" foundations by guesswork or handbook formulas. Foundation engineering will find its place with other branches of civil engineering when as much time and effort are put into it as the relative cost of the foundation warrants.

A similar method of pile analysis based on shear and friction in soils was suggested by G. B. Sowers as a thesis project for students of Case Institute of Technology in 1940. One such study[19] was made, but World War II interfered with further development.

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DISCUSSIONS

OPERATION OF THE CONOWINGO HYDROELECTRIC PLANT

Discussion

BY JOEL D. JUSTIN, E. T. SCHULEEN, AND GORDON R. WILLIAMS

JOEL D. JUSTIN,[3] M. ASCE.—The proper functioning of a run-of-river hydroelectric plant with ample pondage has been adequately covered by the author. In such a system as Mr. Turner describes, served largely by steam plants, a hydro plant like Conowingo in Maryland may be used to reduce the over-all cost of the steam generated energy. To a considerable extent the possession of a plant like Conowingo permits one to place base load on the steam plants and peak load on the hydro plant during the season of low stream flow. During the time of surplus stream flow the allocation of loads to the plants is changed and by preference the hydro plant takes the base load and the steam plants take the peak load.

The great aid which such a hydro plant may be to a system served largely by steam plants is considerable in reducing the over-all cost of steam generated energy. The steam plants are thus enabled to operate on a higher capacity factor. It is evident that with a plant such as Conowingo the pond should be drawn down only a small percentage of the head; otherwise, the decreased head may seriously affect the capacity available at time of peak load and may also decrease the energy generated from the available water supply.

For taking full advantage of rainfall on the watershed (27,000 sq miles), a rather complete warning system is essential—such as has been used at Conowingo, Holtwood, and Safe Harbor, which are companion plants located on the lower Susquehanna River. A thorough system of stream and rain gages is maintained. All gage keepers have telephones and are required to call with regard to any sudden changes. In turn, the gage keepers may be called at any time for special advice as to hydrological conditions. Telephoned records and hydrological advice are quickly tabulated and converted into probable pond inflow.

Knowing that sudden accretion of runoff is likely, the pond may be drawn down to store the freshet waters and avoid wasting kilowatt-hours of energy in flow over the dam. That the engineers at Conowingo have been successful

NOTE.—This paper by Robert E. Turner was published in November, 1947, *Proceedings*.

[3] Cons. Engr., Philadelphia, Pa.

in their predictions is indicated by the fact that the utilization factor at Conowingo (the ratio of the kilowatt-hours actually generated to the kilowatt-hours which theoretically might have been generated) has for many years exceeded 99.5%.

E. T. SCHULEEN,[4] M. ASCE.—The many factors relating to the operation of hydro plants such as those on the lower Susquehanna River which function as part of large steam generating systems have been very ably analyzed by the author. Table 1 calls attention to the large block of effective hydro capacity of 606,000 kw—in these four plants, which capacity, however, is only about 11% of the total capacity in the combined Pennsylvania-New Jersey and Baltimore (Md.)-Washington (D. C.) regional systems. Although use of these plants for peaking capacity and as reserve has been an important factor in their justification, sharply rising fuel costs are placing new emphasis on their energy value.

There can be no dispute with the author's statement (under the heading, "Allocation of Hydroelectric Energy") that: "Knowledge of river flow prospects is the most important consideration in the operation of a hydro plant." Decisions on practically all operating problems hinge on the estimated river flows. The Conowingo flow forecasting methods were developed primarily for a relatively narrow flow range. With this qualification, the use of constant time of peak travel, simple rainfall-runoff ratios, and combined ground and surface runoff recession curves is perhaps justified. When forecasts over a wider range of flows are required, "time of peak travel" curves which vary with stage should be used. Hydrograph plotting to a semilogarithmic scale (as in Fig. 4) possesses the advantage of holding the plot to a convenient size with all probable flows, but it does not permit a ready visual check of the variation in inflow between stations, nor does it allow hydrographs above a confluence to be combined graphically to aid in projecting ahead the hydrograph at a station below the confluence.

Forecasting flows from rainfall is a hazardous procedure, at best, but there is no alternative in obtaining an early forecast when the rain has just stopped or is still falling. After the headwater stations begin to peak, forecasting becomes progressively simpler and more reliable. The most important single factor affecting the accuracy of a flow forecast from rainfall is the determination of the depth of runoff per time interval of rainfall. The author's use of the simple ratios in Fig. 5 may appear to some engineers as a step backward in the science of hydrology. In a theoretical sense this is true, but the practical forecaster, at the time of making his forecast, very seldom has at hand all the data that ultimately become available. Neither does he have time for laborious computations. Hence, a short cut solution such as Mr. Turner has suggested, very well suits the conditions outlined in the paper. However, the author will probably agree that such a solution is not the best obtainable and that the search for better methods should continue.

The author may have minimized the importance of determining the groundwater flow because, under the conditions at Conowingo, ground water does not affect the peak capacity to the extent that it does at Holtwood and Safe Harbor

[4] Hydrographic Engr., Pennsylvania Water & Power Co., Conestoga, Pa.

(in Pennsylvania). With greater incentive in this direction, serious study has been given to the determination of the ground-water component of the total flow, which apparently behaves as though released from a reservoir about 300 sq miles in area, with a season variation in height of between 4 ft and 5 ft, and with a notched weir which controls the outflow entirely as a function of the reservoir height. Consequently, if the elevation of the water surface in this imaginary reservoir (sometimes called the ground-water index[5,6]) is known, and if it is assumed that under the worst possible conditions there would be no more inflow, then the assured outflow hydrograph for a number of weeks to come can be definitely determined.

Referring to Fig. 4 it appears that at the time the rainfall forecast was made on the evening of November 3 sufficient stage data on the upriver hydrographs were available to indicate that the estimates for November 4 and November 5 were too high. The rainfall between Conowingo and Harrisburg (Pa.), subarea No. 1 in Table 2, was relatively low, which probably accounts for no rise in actual flow until the evening of November 4.

The author's example covers a freshet with drawdown possibilities, where accurate forecast of flow makes possible a considerable additional generation. Reliable forecasts are also important for these Susquehanna River plants when the river is relatively stable—particularly if operating schedules indicate the desirability of certain forebay elevations at specified times. Referring to Fig. 3, assume an actual flow of 10,000 cu ft per sec. If the estimated rate of inflow for the period of hydro shutdown, 9:30 p.m. to 8:00 a.m., were 11,000 cu ft per sec, or 1,000 cu ft per sec higher than the actual, the forebay level at Conowingo at 8:00 a.m. would have been only 0.1 ft lower than scheduled, which would not have been serious. At Holtwood, however, where the storage per foot is considerably less than at Conowingo, a similar forecasting error would have made a difference of 0.4 ft in the forebay level.

One of the puzzles presented to a forecaster on the Susquehanna River, as on many rivers in the northern United States, is the effect of ice on river stages. Under ice conditions a rising stage at a particular station may be a legitimate indication of an impending increase in flow downstream. It may also be the the result of backwater behind an ice jam or of ice on the control, in which case the downstream flow would be temporarily decreased. This situation requires constant vigilance and the exercise of good judgment in studying the sequence of gage reports at the affected station and at other stations on the same stream.

Although the author has shown that system economy is affected by the reliability of the river flow estimates, it should be remembered that economy may also be affected by errors in the load forecasts. The errors in the flow and load estimates can be compensating, but they also can be cumulative. Should the load be underestimated and the flow overestimated, or the reverse, considerable readjustment in steam generation schedules might be required.

The author's observation regarding the effect of location of spill along the dam on plant tailwater is pertinent. At Safe Harbor, spill near the powerhouse raises the tailwater twice as much as the equivalent spill 2,000 ft farther

[5] "Analysis of Natural Fluctuations in Ground-Water Elevation," by Carroll F. Merriam, *Transactions*, Am. Geophysical Union, Vol. 23, 1942, pp. 598–603.

[6] "Interpretation of Natural Fluctuations of Ground Water in Terms of River Flow," by Carroll F. Merriam, *Journal*, A.W.W.A., July, 1945, p. 632.

away, which flows for some distance through channels separating it from the tailwater.

Small variations in inflow above power plant capacity are not so troublesome at Safe Harbor as they are at Conowingo because the use of remote controlled regulating gates, instead of crane operated gates, permits closer maintenance of maximum forebay level.

The operators of hydro plants in large steam systems may not always have control over the size of the total load assigned to the station, but they usually do have full responsibility as to the distribution of this load between available units. The author has very clearly explained how the best unit loadings on the Conowingo units are determined. Actual metering of the turbine discharge provides additional opportunities for improving station economy by detecting irregularities in machine performance. Where Kaplan turbines are used, continuous water measurements are helpful in the detection of losses resulting from operation with incorrect relations between gate opening and blade pitch. At Safe Harbor, flow meters connected to scroll case piezometers not only provide a large part of the record of river flow, but also aid in checking unit performance and are the basis of a scheme whereby the operator receives a light signal when the incremental equivalent justifies a change in the number of units operating.[7]

Working on the same river with Mr. Turner, the writer is impressed with the importance of cooperation. By proper coordination considerable mutual assistance can be rendered from one company to another. When plants are closely located, as in this case, so that the time of wave travel from one to another is very short, coincidence in hours of daily water use permits much smaller fluctuations in forebay level in the lower plants than would be possible if each plant were the only plant on the river.

GORDON R. WILLIAMS,[8] ASSOC. M. ASCE.—Hydrologic engineering is not only of vital importance in the original design of hydroelectric projects, but it can play an important part in the profitable operation of such projects. The author has used modern hydrologic techniques effectively to develop forecasts of mean 12-hr flows for 10 days or more following a storm. The problem differs from ordinary flood forecasting in that the entire runoff hydrograph must be estimated rather than merely an instantaneous peak stage and discharge.

The statement is made that the methods adopted are empirical rather than theoretical. It is safe to state that all hydrologic relationships are empirical in that no exact mathematical law can be devised to express such relationships. The degree of refinement used in developing and applying the relationships depends on the accuracy of the basic data, the time and funds available for computing the results, and the benefits to be obtained from a greater degree of refinement. The test of any empirical procedure, hydrologic or otherwise, is whether or not it gives the desired results. Apparently the river flow-forecasting method as developed and as modified by judgment and experience has proved satisfactory. The writer offers the following suggestions with the hope that they may be helpful if later refinements are necessary.

[7] "More Power Through High Operating Efficiencies of Hydro Electric Plants," by W. B. Hess and J. M. Mousson, *Bulletin*, Edison Electric Inst., July, 1941.

[8] Associate, Knappen Eng. Co., New York, N. Y.

Runoff is considered a residual of rainfall after losses have been satisfied. Losses for various seasons are relatively constant; and, therefore, if runoff is expressed as a ratio to rainfall as shown in Fig. 5, there is a wide variation in the ratio with the depth of rainfall. Fig. 5 is based on a study of the relation between rainfall and runoff for the full duration of the storm and makes no allowance for the fact that percentage of runoff may also vary inversely with the duration of the storms. More consistent results could be obtained by computing losses as definite quantities and relating them to the duration of the storm. For example, if the basin rainfall were 3 in. in 24 hours and the total surface runoff were 1 in., the loss rate for that particular storm and season would have been 1 in. in 12 hours. The loss rate might also be correlated with the total antecedent rainfall for periods up to 10 days or more prior to the storm. If the foregoing procedure were substituted for that used in deriving Fig. 5, it is possible that a more consistent correlation would be obtained and that there would be less need for depending on judgment in applying the relationships to future storms.

In determining river discharge from rainfall excess, distribution graphs with runoff expressed as a percentage of the total runoff are illustrated in Fig. 6. There does not seem to be any advantage in the use of the distribution graph rather than the normal unit hydrograph from which the discharge for any storm is obtained directly by multiplying the ordinates by the inches of runoff. Since the desired results should be in average flows for 12-hr periods, typical unit hydrographs could be set up as "hystograms," or bar graphs, in which each bar represents the average discharge for a 12-hr period and the total of the bars is equal to 1 in. of runoff from a 12-hr period of rainfall excess.

The author mentions the difficulty of obtaining an average distribution graph which is representative of various storm distributions over the subareas. This difficulty is understandable when it is realized that subareas as large as one of 9,960 sq miles (that of the North Branch above Wilkes-Barre, Pa.) have been considered. Two courses of action are suggested—one is to increase the number of subareas and the other is to derive several distribution graphs for each subarea. The purpose of having several distribution graphs (or unit hydrographs) would be to have one applicable to the case where the rainfall was concentrated near the outlet, another for the case where the rainfall was concentrated near the headwaters, and possibly a third to take care of the case where the rainfall was concentrated in the center of the subarea.

The recession curves shown in Fig. 7 have been separated on a seasonal basis; and, without access to the basic data, it is not clear why there should be this difference. The recession for the first 10 days to 15 days represents depletion of channel storage and except for the volume of storage taken up by ice the amount of storage available in the winter should be essentially the same as that in the open-water months. Periods of heavy runoff in the winter are often followed by cold waves that cause runoff to be temporarily ponded in numerous small ice jams. This runoff is later released and may account for the fact that the curve for fall and winter is higher than the curve for spring and fall after an interval of 15 days. For periods of from 15 days to 35 days, the curves would normally represent ground-water flow rather than surface runoff coming out of channel storage.

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DISCUSSIONS

THE SIGNIFICANCE OF PORE PRESSURE IN HYDRAULIC STRUCTURES

Discussion

BY WILLIAM P. CREAGER, J. S. KENDRICK, ADOLF A. MEYER, E. MONTFORD FUCIK, A. C. R. ALBERY, AND W. H. R. NIMMO

WILLIAM P. CREAGER,[12] M. ASCE.—A meritorious effort has been made by the author to induce the engineering profession to abandon a hidebound concept of uplift pressures, which, time and again, in current publication has been proved erroneous by deduction and conclusive tests. When a book[13] on this subject was published by the writer with others in 1945, it was hoped that its advocacy of 100% uplift area would be adopted. However, the writer has since been called upon repeatedly to review plans where less than 100% uplift area has been assumed.

The design of the Tennessee Valley Authority (TVA) dams embodied the assumption[14] that uplift pressures would be applied over only two thirds of the area of the base of the dam—despite the fact that, in some of its dams, the foundation consisted of calcareous rock with approximately horizontal bedding planes, considered favorable to uplift. In the writer's discussion of this paper,[15] he protested such an assumption to no avail, although the author of that paper confessed that it could not be defended experimentally and was adopted because it represented current practice.

Other engineers, including those of prominent government agencies, at present assume a full hydraulic gradient over one half or two thirds of the area of the base. When they are half convinced that this is erroneous, they explain that full hydrostatic pressure over half the area of the base is equivalent to half the full hydraulic gradient over 100% of the base area. This theory is not true at all, for the simple reason that, although the hydrostatic pressure

NOTE.—This paper by L. F. Harza was published in the December, 1947, *Proceedings*.

[12] Cons. Hydr. Engr., Buffalo, N. Y.

[13] "Engineering for Dams," Vol. II, by Julian Hinds, William P. Creager, and Joel D. Justin, John Wiley & Sons, Inc., New York, N. Y., 1945.

[14] "Structural Features of Hydraulic Structures," by Ross M. Riegel, in "Design Developments—Structures of the Tennessee Valley Authority: A Symposium," *Transactions*, ASCE, Vol. 111, 1946, p. 1162.

[15] *Ibid.*, p. 1202.

at the upstream side can be reduced by grouting and drainage, the hydrostatic pressure at the downstream side cannot be reduced at all, but must remain equal to the tailwater pressure. Thus, if uplift is assumed to act over 100% of the area, the downstream toe is subjected to full uplift pressure of tailwater.

The paper has been confined to the subject of pore pressures. However, since pore pressures are induced by the hydrostatic pressures corresponding to the hydraulic gradient, the two are closely related and a short discussion of the latter feature would seem to be in order.

The writer's opinion[15] of the proper assumption for the hydraulic gradient is briefly as follows: For a dam without a cutoff, drainage, or upstream blanket, the hydraulic gradient varies from full headwater pressure at the upstream side to full tailwater pressure at the downstream side. The author has indicated reasons why thin dams have remained stable under such conditions and the writer will add his bit hereinafter. For a dam with a grouted cutoff supplemented by adjacent drainage, the hydraulic gradient should be considered to vary uniformly between: (a) Full headwater pressure at the upstream side; (b) tailwater pressure plus a fraction p of the differential head on the dam at the drains; and (c) full tailwater pressure at the downstream side.

The actual hydraulic gradient under ten dams with drains was published by Ivan Houk, M. ASCE, in 1932.[16] In all but one of the dams having grout curtains or drains the value of p ranged from about one sixth to two thirds. More recent tests on TVA dams showed the possibility of obtaining practically a zero value for p under best conditions. The actual value of p depends upon the nature of the foundation and the effectiveness of the grouting and drainage, which are not known beforehand. Conservatism is therefore necessary. The use of the values of p between one half and two thirds, depending upon conditions, should be adhered to.

The author has given several "mitigating circumstances" which account for the fact that thin dams, subjected to uplift, have not failed. The Barker Dam in Colorado is such an example. Before rehabilitation it had a base equal to about 70% of the height, at which there was no effective foundation treatment and in which there was pronounced leakage through the foundation and horizontal construction joints, all of which "pointed to the probability of the existance of large uplift pressures, not considered in the original design."[17] In this dam, neglecting silt, ice, and earthquake pressures, any uplift in excess of about 30% would put the resultant outside the middle third.

The author's mitigating circumstances assisting stability are not credited assumptions in design. However, there is one item which is and must be tacitly assumed to exist. An orthodox design of a dam neglects consideration of possible tensile strength in either the dam or the foundation. For low dams on massive rock foundations grouting and draining the rock and draining the dam are not resorted to. In such a dam the hydraulic gradient may exhibit straight-line variation from headwater to tailwater, and full uplift over 100% of the area must be assumed to exist. However, such assumptions are never used under these circumstances.

[16] "Uplift Pressures in Masonry Dams," by Ivan Houk, *Civil Engineering*, September, 1932, p. 578.
[17] "Rehabilitation of Barker Dam," by Raymond E. Davis, address, before the Power Division, ASCE, Annual Meeting, 1948.

In a well-built dam on massive rock foundations, with a continuously filled reservoir, the "cohesive" or tensile strength of the dam and the foundation is the only one of the author's mitigating circumstances which has any practical significance. It must be remembered that, if the resultant is outside the middle third, and tensile strength is not developed, full 100% uplift equivalent to headwater pressure will exist over the area in tension and failure will result by progression.

Actually, tension does exist in massive rock foundations and in the horizontal construction joints in the dam. Assuming this tension to have a value of 85 lb per sq in., it will resist 200 ft of hydrostatic uplift head. It would, therefore, seem that all dams more than 150 ft high should be provided with a grout curtain and complete drainage, even if erected on massive rock foundations.

J. S. KENDRICK,[18] Assoc. M. ASCE.—That uplift acts on the entire submerged body of a hydraulic structure has been demonstrated in convincing fashion by the author. In so doing he has challenged, in the paragraph following the "Synopsis," two concepts which most authorities have treated as axiomatic:

> "First, that 100% uplift area cannot be true because a large proportion of the base area must be in contact to support the load; and, second, that many or most existing dams would have failed if the 100% uplift area doctrine were true."

He had disposed of the first concept most effectively, but further examination of the second concept may be desirable. This examination discloses one or two modifying factors in the author's deductions, but it supports rather than detracts from his argument.

The concept that existing dams would have failed if the uplift area were 100% usually is based on the assumption that the foundation reaction is distributed in accordance with a linear pressure diagram. If the dam and foundation are considered as an elastic unit, this distribution cannot exist. However, to start with, even if it did exist, it does not follow that a dam might fail as a result. Uplift, or pore pressure, takes time to develop, and the stresses beyond the elastic limit may be taken care of by plastic flow, or by local adjustment of deformations in accordance with the elastic properties of the dam and foundation.

Quite apart from these adjustments, there is no reason to suppose that good concrete, or a well-made concrete-to-rock bond cannot resist some tension. A 200-ft-high dam with a base width 0.7 H and a full buoyancy would have a tension of only 50 lb per sq in. at the heel, with linear distribution of reaction and all water load applied at the upstream face.

However, Mr. Harza has shown that the water load is not all applied at the upstream face, although he has not followed his development to the conclusion indicated by his treatment of concrete as a porous medium, in which flow conforms with D'Arcy's law. Conditions of flow may be represented graphically

[18] Dist. Engr., Water Rights Branch, Dept. of Lands, Victoria, B. C., Canada.

by a flow net. Fig. 12 shows the flow net for a triangular section with a base width equal to 0.7 H. The base is assumed to be impervious. The flow line FF′ is intersected normally by equipotential lines similar to EE′. The element of water pressure dp is equal to $w\ h\ dh$ for a unit slice of the dam and is commonly assumed to have a moment about point A of $y_1\ dp$. Actually, the force dp is dissipated gradually over the flow path FF′, and the moment about point

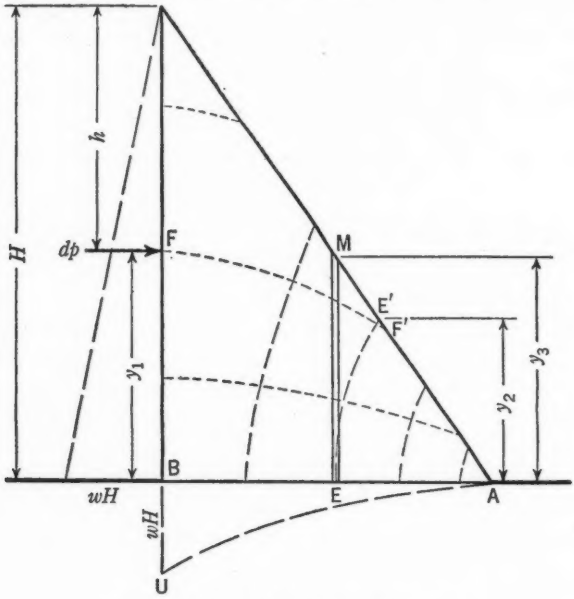

FIG. 12.—FLOW NET FOR TRIANGULAR DAM

A is obviously less. This is the well-known but ill-understood "overturning moment."

Considering a vertical element EM of the dam itself with a cross-sectional area dA, the buoyant force on this element would be $w\ y_3\ dA$, if it were completely submerged. This force would be the equivalent of an "uplift" at point E produced by a triangular uplift diagram with full hydrostatic pressure at point B. However, line EE′ is an equipotential line, or a line of constant piezometric head in the flow net. It follows, therefore, that the actual buoyant force will be $w\ y_2\ dA$, or less than the full buoyant force by the ratio y_2/y_3. This corresponds to an uplift diagram of the shape ABU.

The author's reasoning, under the heading, "Application to a Concrete Dam," that leads him to the deduction of full buoyancy, depends on the assumption that "* * * the reduction of pressure at adjacent elevations follows a similar curve"—an assumption not supported by the flow net. The convex uplift diagram ABU exerts less moment about point A than does the author's triangular one, and helps to explain the satisfactory performance of existing dams where the base width is less than 0.7 H, thus lending additional support to Mr. Harza's deduction of 100% uplift area. If the foundation is assumed

to be pervious, and the usual practice of curtain grouting at the heel is carried out, the freely draining base will cause all the flow lines of the flow net to assume steeper slopes, further reducing the "overturning moment," and even transmitting some of the horizontal water load directly to the foundation.

The writer considers that further development of the analysis of gravity dams will depend on considerations of elastic characteristics of dam and foundation, and on applications of forces by the flow net principle. However, such considerations lie outside the proper limits for discussion of this important paper.

ADOLF A. MEYER,[19] M. ASCE.—Fourteen years ago Karl Terzaghi, Hon. M. ASCE, published his papers on the effective boundary porosity of concrete.[20,21] From a comparison of the compressive strength of concrete cylinders tested under confining pressure, with the confining pressure acting either on a jacket on the surface of the cylinders or in the pores of the cylinders, he concluded that the effective boundary porosity is near unity. He did not fail to point out the full implication of this conclusion on the concept of uplift in concrete dams, but a review of the textbooks published in recent years and of general design practice indicates that little credence was given to these findings. However, a certain uneasiness about the subject of uplift definitely exists in wide circles. For this reason the bold presentation by the author is to be welcomed very much. Its attention to the fundamental physical phenomena involved should help to bring forth clarifying discussion, with the result that more realistic assumptions will be introduced in the design of future dams.

Much evidence is on hand to support the author's description of concrete as a porous and permeable material. The concept as such was introduced more than thirty years ago. Reliable permeability tests were made very much later, in particular those for the Boulder Dam (in Arizona-Nevada) investigation to which the author refers. Since air-entrained concrete has come into commercial use, much careful research has been conducted on the porosity of concrete. There should be no further difficulty in accepting mass concrete as a porous and permeable material, with the flow of water through a concrete structure following Darcy's law approximately. Saturation is a slow process. For instance, at the Fontana Dam of the Tennessee Valley Authority in North Carolina, a piezometer tip cell, placed only $3\frac{1}{2}$ ft from the upstream face, registered the first water pressure two months after the rising reservoir level had passed the level of the cell. By that time, the reservoir had risen almost 200 ft above the level of the cell. Another ten months were required to increase the pore pressure to the intensity that apparently corresponds to its stable value within the flow net, which is controlled by a drain curtain 30 ft from the water-exposed surface. The permeability coefficients approximated from these observations are $k = 0.75$ ft per yr for the first condition and $k = 3$ ft per yr for the second. The concrete at the upstream face contains 1.25 bbl of cement per cu yd,

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[20] "Beanspruchung von Gewichtstaumauern durch das strömende Sickerwasser," by Karl Terzaghi, *Die Bautechnik*, July 6, 1934, p. 379.

[21] "Zur statischen Berechnung der Gewichtstaumauern," by Karl Terzaghi, *ibid.*, October 19, 1934, p. 589.

whereas the main body for the second condition contains 0.8 bbl per cu yd. These coefficients are somewhat higher than laboratory data tested on similar mixes.

It is easy to follow the author's reasoning on buoyancy of granular material and its application to almost all types of rock formations. Even when disregarding the boundary porosity of the rock itself, about which general statements should be avoided because of the radically different origin of rocks, there is no doubt that practically all rock structures are jointed and that the joints are pervious. However, when the author applies his reasoning to concrete, he introduces a concrete lacking in all tensile strength. This assumption certainly does not correspond to reality. The author actually refers in several instances to "cohesive strength" or to "some strength along the surface of failure," the existence of which he admits cannot be denied, but in his reasoning about pore pressures and buoyancy, all references to this quality of concrete are omitted. As Mr. Harza otherwise shows a keen interest in the basic physical characteristics of the materials involved, his departure from this course in this particular important instance is most unusual. Every concrete dam built by modern methods and with cement now commercially available depends for its action as a gravity cantilever on tensile strength. The writer knows of no large dam where the placing temperature plus the temperature rise from heat of hydration does not exceed the final stable temperature. The unavoidable cooling, whether natural of forced, brings about a decrease in volume which in the restraint zone of the dam where it cannot take its full course will be compensated for by internal tensile stresses. Therefore, in one important phase the tensile strength of concrete must be relied upon. How can it be considered nonexistent in another?

Professor Terzaghi, faced with a high effective boundary porosity, concluded that the real tensile strength of the effective area of cement paste must reach astounding values—in the order of from 10,000 lb per sq in. to 50,000 lb per sq in. He compares the action of the paste with threads of glass, which by experiment have shown tensile strengths of still much higher values. It is the writer's opinion that the promulgation of the use of an effective area for uplift much larger than has generally been accepted would best be served by carefully planned boundary porosity tests with confining pressures much higher than the tests available to Professor Terzaghi. According to Douglas McHenry,[22] such tests are being carried out by the Bureau of Reclamation, and it will be of great interest to learn their results.

Although the author follows a line which differs very much from the experiences shown by triaxial load tests, his recommendation for design is the same as the conclusions derived from those tests. In its practical application, near unit boundary porosity requires 100% area application of uplift. It is true that the last few units of this percentage remain undefined. However, within the accuracy obtainable in the analysis of a dam, there is no practical gain in squabbling over the exact, specific value of the boundary porosity.

The author states that with this uplift assumption a base width of from 0.831 H to 0.846 H is required to keep the resultant within the middle third.

[22] *Proceedings*, A.S.T.M., Vol. 45, 1945, p. 793.

After reading his explanation on flow through the dam, it is logical to ask to what type of flow net does this relation apply. It is obviously correct for the flow net shown in Fig. 9(*a*). It presupposes a dam without contraction joints, without galleries, without transverse cracks, and with a rock foundation at least as impermeable as the concrete above. Few dams built in the past approach such a condition. Since the permeability of even the best type of rock foundation is far greater than the concrete permeability, this difference will have a decisive influence on the flow net within the dam. Professor Terzaghi in 1934 showed by a few applications how the location of the resultant will be influenced by the foundation permeability. Much more direct and decisive will be the presence of drainage provisions within the body of the concrete itself. Many actual observations of pore pressures clearly show how readily the flow net corresponds to these opportunities for pressure relief. For future dams the designer can prepare for a flow net that will give him the maximum utilization of his materials.

In his final statement, the author shows himself to be in complete disagreement with this line of approach. "Consideration should be given to a stable section without drainage * * *," because "The value and success of drainage are questionable" and because seepage weakens concrete and rock. How the author can question the "value" of drainage, after explaining its action most effectively, is difficult to understand. As to the effectiveness, there are too many observations on successful installations to allow any doubt that effective drain curtains can be provided. A more realistic inquiry might ask: Will the effectiveness last for all times? If the drain curtain is not maintained, it might stop functioning properly, but there is no doubt that with proper care—and little care that is—it can always be kept functioning. Therefore, this periodic check and the possibility of small maintenance work at infrequent intervals cannot be used as binding arguments against the use of a drain curtain. Otherwise, it could just as well be concluded that it is unwise to build steel bridges in the future because they would fail some day if allowed to rust.

That seepage weakens concrete and rock is, of course, a truism; but, like any statement of this type, its terms need qualifying definitions. What is harmful seepage? The author agrees that seepage occurs through a dam with a base width of 0.8 H. Many successful dams of the multiple arch and slab type have drainage passages with a length of from only 0.02 H to 0.03 H. Assuming the same concrete quality, their seepage volume must be from twenty-seven times, to forty times as much as the unit seepage in the nondrained gravity dam. If the experience record of the dams with high seepage rates is satisfactory, does it then show good judgment to add a safety factor of 27 or 40 on and above a satisfactory condition?

The writer does not advocate the use of drain curtains in gravity dams in a position which would anywhere reproduce the short drainage drops in slab dams. Reasons of temperature control usually make it undesirable to use a rich mix in gravity dams; the permeability coefficient will therefore be considerably higher than that for the rich mixes used in slab dams. However, based on the experiences gained from these slab dams, with conservative allowance for different permeability coefficients, location of a drain curtain can be

selected which will not affect the permanence of the structure but will decisively improve its stability.

By his interesting presentation of concrete dams as porous and permeable bodies, the author is helping to extend a better understanding of the structural action of gravity dams. On studying his paper, the thought immediately follows how well the profession would be served if much of the pertinent data affecting this discussion could be made readily available—especially boundary porosity tests and actual measurements of pore pressures, uplift intensities, and permeability coefficients of mass concrete and of different rock foundations, both in natural condition and after grouting. Such data will help to clarify the zones where opinions differ. The writer believes, however, that there are enough factual data to adopt uplift assumptions which express unit boundary porosity. Furthermore, enough observations are available to justify the design of dams with planned drain curtains, extending into the foundation rock and up through the full height of the concrete body. With a flow net controlled in this fashion, durable structures with conservative safety characteristics can be built with a base width of from 0.7 H to 0.76 H, the range being controlled by unit weight and the location of drain curtain. This location in its turn must take into consideration the permeability coefficients of the selected concrete mix and of the treated and untreated foundation rock.

E. MONTFORD FUCIK,[23] ASSOC. M. ASCE.—The imaginary conception of concrete as composed of vertical cylindrical elements with pores at top and bottom is apparently the basis for the theory and the conclusions expressed in the paper. If this conception is in error the conclusions must necessarily be so.

To date all recorded data have been concerned with the original sand, gravel, and cement particles. The cementing together of these particles obviously causes areas of cemented contact on top, bottom, and sides which can be assumed to exclude water pressure. Such contact areas when they occur on top and bottom would reduce the area of the original particles subject to uplift.

It would appear, however, that, once cementing has taken place, the original particles have lost their identity and are of no greater significance than any other subdivision of the material which can be logically imagined to exist. To argue otherwise would be to endow the original particles with the power of memory.

The author's imaginary subdivision of concrete into vertical cylinders with pores at top and bottom was based largely on the conception of pore space in the form of more or less globular volumes connected by microscopic channels of less importance. Apparently, relatively few (perhaps about 2% by volume) of more or less globular pores exist in concrete having about 12% of voids, of which the cement paste has about 50% voids; and most of the voids in the cement paste consist of microscopic channels leading between and around the cement grains.

Globular pores to the amount of 2% might be ample to support the author's reasoning, but this theory would seem to be improved by starting with contact areas of cement grains instead of globular pores. Suppose that such contact areas occur on the top or bottom projected area of the cement grains and that

[23] Harza Eng. Co., Chicago, Ill.

an imaginary vertical column incorporating each area of top or bottom contact is erected. Then such columns within a distance of a few cement grains will end in sufficient voids at top and bottom to terminate the column. Thus, the entire mass would be composed of vertical cylinders with top and bottom exposed to water pressure, as arrived at exactly by starting with the author's conception. Within a distance of a few cement grain diameters up or down, a surface of 100% projected uplift area would be formed. The imaginary particles making up the mass would now consist, just as before, of cylinders joined solidly to each other on their lateral faces instead of at top and bottom. The lateral contacts would not reduce the area of uplift below 100%, and only very small, almost microscopic, departure vertically would be needed to incorporate a 100% pore area.

Obviously a specimen would not break in this manner, but there need be no relation between surface of fracture and surface of effective uplift pressure. It is common knowledge that a fracture surface will pass through practically poreless aggregate pieces, although such a surface obviously would not be the surface of maximum porosity. This occurs when the aggregate pieces are weaker without pores than the cement paste is with its pores. In other words, a fracture picks the weakest section, whereas the uplift surface is determined by the maximum pore area, not necessarily related to the weakest section.

To arrive at 100% uplift area it is necessary to consider only joint seepage, rather than internal pore pressure in the mass of the concrete between horizontal joints. Conventional reasoning now assumes a pressure drop in horizontal joints reducing from reservoir pressure at the back to zero (or tailwater) at the face. The sole difference is that it is commonly assumed to be applied over, say, two thirds of the horizontal area. Joints are made with cement paste or mortar. To produce uplift over 100% of the area, it is only necessary for joint seepage pressure to spread through the cement or adjacent mortar above and below the joint for a sufficient distance (amounting to the diameter of a few cement grains) to incorporate pores of 100% horizontally projected area. A zone adjacent to the joint would doubtless be under pressure for sufficient thickness. Pressure need be built up in only one such joint in order to subject the entire volume above that joint to 100% uplift. This would be true if the pore pressure existed only in a single horizontal joint or in adjacent joints, because the elementary vertical cylinders of which the mass is assumed to be composed can extend from one horizontal joint to the next joint above or below it instead of from one pore to the next, without invalidating the reasoning.

Many modern, carefully built dams show efflorescence along horizontal joints, indicating that seepage often penetrates horizontal joints in a short time even if it may take many years to penetrate the body of the concrete. In fact, on uplift pressure tests made on several dams, usually at the horizontal joints, indicate that not many years are required to reach a state of equilibrium. The pressure line when determined is usually erratic and does not conform closely with the convenntional straight line from reservoir pressure at the back to zero at the face. The author, however, is concerned only with the area of uplift, not its magnitude.

The writer agrees with the author's conclusion that uplift must be considered as acting over 100% of the horizontal area. In view of the innumerable ex-

amples of successful concrete dams designed using only three fourths or two thirds of the area as affected by uplift, the conclusion can be drawn that the pressures assumed were higher than those that actually occurred. The net result is the same whether one uses two thirds of the area and 100% headwater pressure, or two thirds headwater pressure and 100% of the area.

A. C. R. ALBERY,[24] ESQ.—While engaged on the design of a dam of the round-head buttress type, it was somewhat disconcerting to read the author's contention at the end of his paper that the round-head design was based on a fallacy. However, the writer has come to the conclusion that Mr. Harza's argument, although convincing for horizontal planes and for vertical planes at right angles to the lines of seepage flow, cannot be applied to vertical planes parallel to the lines of seepage flow.

The argument that uplift on any horizontal plane is really a buoyancy effect is a good one, and the writer agrees that the uplift is probably equivalent to the hydrostatic pressure acting on 100% area. The word "equivalent" is important. The uplift is not an actual hydrostatic pressure acting on 100% area, but is an aggregate buoyancy effect acting throughout the whole mass of concrete above the plane under consideration. A similar effect is produced by the steady drop in pressure along the lines of seepage flow. There is no such effect, however, on vertical planes parallel to the lines of seepage flow, and the author's argument cannot be used to prove it. On such planes the hydrostatic pressure will act only on the area of voids cut by any particular plane, which, as assumed in the paper, may be only about 12%.

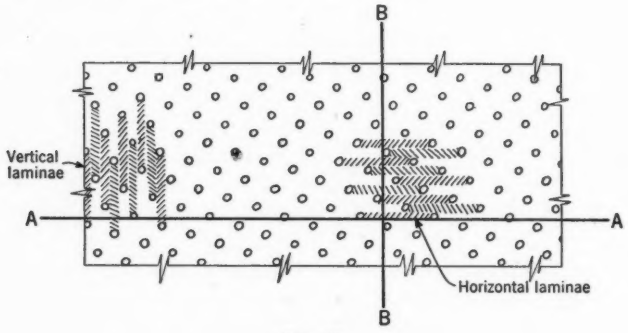

FIG. 13

The writer has found it easier to visualize the action of the uplift force by idealizing the conditions. Imagine a number of minute, straight, horizontal tubes passing through an otherwise impermeable dam in a symmetrical arrangement, as shown in Fig. 13. Water is forcing its way through so that the pressure drops from the full head at the upstream face to zero at the downstream face. Any horizontal plane such as A–A will only pass through a certain number of the tubes, and the hydrostatic pressure on that plane will only act on the area of the tubes cut by the plane. On the other hand, the dam may be divided into a number of vertical laminae, each having a tube above and a tube below. With water at the top and the bottom each lamina is, in effect,

[24] London, England.

submerged and its effective weight will be its submerged weight. In the case of a triangular dam, the use of the submerged weight in the calculations gives the same result as using the actual weight and allowing for an uplift pressure varying from full head to zero and acting on 100% area. It is, nevertheless, not the same thing.

By assuming interflow between the tubes a similar argument can be used (see Fig. 4(*c*)) to show that there is a horizontal "buoyancy" effect on vertical planes perpendicular to the tubes because of the drop in pressure in the direction of flow.

Now consider any vertical plane parallel to the lines of flow, such as B–B. Once again the hydrostatic pressure will act on only the area of the tubes cut by the plane. However, if the dam is divided into a number of horizontal laminae, each having a tube at both sides, the water pressure on each side will be the same and there can be no resultant force due to buoyancy or differential pressures. The author himself put the argument very clearly when, referring to the experiments of S. Leliavsky Bey, he stated (under the heading, "Experimental Evidence"):

> "The concrete cylinders joining adjacent pores will be subjected, therefore, to the same pressure at each end and will have no tendency to split apart or any tendency whatever to move as the result of water pressure in these pores. No wedging action or longitudinal stress can result from this pore pressure as long as the pressure is the same at each end of any elementary cylinder."

The writer believes that the probable effects of the pore pressure within concrete may be summarized as follows:

1. The water pressure within the pore spaces in concrete creates an aggregate buoyancy effect, which, in the case of a triangular dam, is equivalent to an uplift pressure acting on 100% area of horizontal planes.
2. Provided that a pressure gradient exists through the dam falling from full head to zero, there may be a similar effect on vertical planes perpendicular to the lines of flow, which amounts to hydrostatic pressure acting on 100% area. If there is no pressure gradient, there will be no such effect.
3. On vertical planes parallel to the lines of flow there is no differential pressure effect, and the hydrostatic pressure within the concrete acts on only the area of the pore spaces cut by any true plane, which with good concrete may be only about 12% of the total area.

The plane in Fig. 11 is almost parallel to the lines of flow, and Eq. 13 for the bending moment in the wing of a round-head buttress dam due to the so-called wedge effect should be more nearly

$$M = \frac{12}{100}\, w\, \frac{h\,(\mathrm{BC})}{2}\, \frac{2}{3}\,(\mathrm{BC}) = \frac{w\,h}{25}\,(\mathrm{BC})^2 \ldots\ldots\ldots\ldots (14)$$

Such a force may reasonably be resisted by the inherent tensile strength of the concrete. Preferably, however, it should be provided for in the design of the head. Even if it is not agreed that the hydrostatic pressure acts over only a small percentage of the area, the total area affected by seepage flow can be greatly reduced by providing a series of small drains a few feet back from the upstream face and extending the entire height of the head.

The magnitude of the hydrostatic pressure may also be slightly reduced over the bulk of the head by employing a facing concrete that is rich compared

with the remainder. The stresses in the head of a round-head buttress dam are usually very low, and the bulk of the head may be of a weak concrete. For the same workability the rich facing concrete can have a much lower water-cement ratio than the weak concrete. The total uncombined water per unit volume of concrete will also be less. Consequently, the percentage of voids will be less and the resistance to seepage flow greater. The pressure drop through the facing concrete will be relatively large, and the maximum intensity of pressure over the bulk of the head behind will be reduced.

In conclusion, the writer believes that this paper, far from condemning the round-head buttress dam, actually strengthens the argument for its adoption. If the author's theory of full buoyancy is accepted, the profiles of gravity dams may become more conservative, and any type of buttress dam will show a greater relative economy. Alternatively, the small total area over which uplift can act will give the buttress dam a greater relative factor of safety against sliding or overturning.

W. H. R. NIMMO,[25] M. ASCE.—Many engineers who have designed dams for a percentage of full theoretical uplift doubtless realize that the percentage applies to the intensity of pressure and not to the area on which it acts. Nevertheless, the author's demonstration of that fact is timely in the interest of clear thinking. The writer is entirely in agreement with the conclusions stated by the author, except the last one regarding the value of drains.

In combating a proposal to build a vertical wall supported by struts against the upstream face of a recently constructed gravity dam to prevent the possibility of leaching of the cement from the concrete, the writer investigated the percolation to be expected through the dam. The dam was built for the dual purpose of flood mitigation and water supply and the reservoir will usually be filled to 105 ft above the minimum level of tailwater, where the dam is 116.5 ft thick, with 12.5 ft of 6-bag concrete at the upstream face and with 4-bag concrete at the remainder of the section. There are 6-in. diameter vertical drains, spaced 10 ft apart, 16 ft from the upstream face. The coefficients of permeability, for the 6-bag and 4-bag concrete, respectively, were found by experiment to be 20×10^{-12} cu ft per sec per sq ft and 800×10^{-12} cu ft per sec per sq ft for a head loss of 1 ft per ft.

Assuming that the concrete is homogeneous and that percolation is uniformly distributed, the time required to saturate the full thickness of the dam was found to be about 270 years. However, percolation along a horizontal construction joint might be much more rapid and lead to saturation of the dam at some levels in a comparatively short period. Computation indicated that the drains will intercept 96% of the water, thus reducing the saturation time (assuming percolation uniformly distributed) to about 5,000 years. In this case it seems that the drains may appreciably relieve uplift.

Denser concrete in the interior of a dam, or horizontal cracks extending inward from the upstream face, may entirely upset the usual assumption that uplift intensity varies linearly from headwater to tailwater. Thus, full reservoir head may act over an appreciable area at some levels. Under these circumstances, drains will be valuable both in reducing uplift pressure over the area downstream from them and in acting as indicators of any abnormal leakage, which may then be stopped by drilling and grouting.

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DISCUSSIONS

UNUSUAL DESIGN PROBLEMS—SECOND TACOMA NARROWS BRIDGE

Discussion

BY O. H. AMMANN, CHARLES A. ELLIS, AND F. H. FRANKLAND

O. H. AMMANN,[2] M. ASCE.—Credit is due the author and his associates responsible for the development of the redesign of the Tacoma Narrows Bridge in the State of Washington for their efforts to explore the elements which influence the dynamic motions in suspension bridges under wind action, and to produce a design that can be judged adequate and within reasonable economic limits on the basis of present knowledge.

In these efforts the designers were fortunate in having at their disposal two essentials for research work not usually available to engineers planning a bridge of this type—namely, ample time and sufficient funds. With construction funds partly available, and with important parts of the old structure utilizable, in addition to proved economic justification, the eventual reconstruction of the bridge was a foregone conclusion. However, due to wartime conditions it had to be delayed for several years, which offered an opportunity for extended research. The research expenditures to which the federal government liberally contributed could be justified on the ground that the findings were expected to be of value for the design of suspension bridges in general. Such expenditures could not be economically justified for individual projects, even if funds could be raised as a preliminary to financing a project (which, as a rule, is not the case). It is very doubtful, in the opinion of the writer, whether the results of the research work so far obtained and applied in the form of new features in the redesign of the Tacoma bridge may be utilized by the profession in designing other suspension bridges without further extended research.

The author properly points out that, for a more complete exploration of all the complex elements contributing to the behavior of suspension bridges under wind action, more research is needed. Among others, he mentions two most important elements on which so far there has been only very inadequate exploration—namely, the damping characteristics of the structure, and the character of the winds as they exist in nature, which it is impossible to reproduce

NOTE.—This paper by Charles E. Andrew was published in December, 1947, *Proceedings*.

[2]Cons. Engr., New York, N. Y.

in a wind tunnel. These and other important factors are so complex, so widely variable in different structures and localities, and so difficult to explore adequately, that one may well doubt whether it will ever be possible to arrive at generally applicable design directives based on scientific research such as so far has been applied in the case of the Tacoma bridge.

The author confines himself to a general qualitative outline of the redesign of the Tacoma bridge. Although he points out the major features that contribute to the adequacy of the design, with respect to wind action, he does not attempt even a rough appraisal of their effectiveness and economic merits, which are of prime importance to the design engineer. Unfortunately, the paper does not contain all the essential design data—such as dimensions of members, sectional areas of members, and weight of structure—on which the reader can base such an appraisal. In an article published elsewhere the author gives some of these data (41).[2a] It may be assumed that they have not been materially changed, except in one important feature—namely, the addition of a second lateral system in the plane of the bottom chords of the stiffening trusses.

One who has thoroughly familiarized himself with the nature of the problem of designing suspension bridges against dynamic wind action, and has closely followed the research work in this respect, cannot but get the impression that the effect of what may be called streamlining provisions in the design of the Tacoma bridge have been grossly overemphasized in this paper. It cannot be doubted that these provisions will contribute to the margin of safety of the structure, more particularly against the extremely remote possibility of dangerous torsional oscillations under winds of high velocity. On the other hand, there is nothing to indicate that they contribute any major share to that margin, or that the structure, as otherwise designed, would not be adequate without them.

If, as sound judgement would seem to permit, the streamlining of the rail section is disregarded as a wholly negligible factor, major reliance for streamlining must be placed on the grid covered, open slots in the roadway slab, which, according to the wind tunnel tests on the model, appear to influence the behavior of the structure favorably. Aside from the question whether such open slots may prove to be objectionable for traffic, or as a feature to be permanently maintained on the bridge, there is still the unsolved question of whether, in the actual structure under actual wind conditions, this provision can be relied on to furnish any appreciable share of safety, not only against dangerous torsional oscillations, but also particularly against such relatively moderate vertical motions as would be noticeable to the traveling public. The author expresses the opinion that it is entirely probable the design without the slotted deck also would be satisfactory. The writer agrees with this opinion, even to the extent of expressing full confidence that the bridge would prove adequate without the slots, as the latter merely represent an additional safeguard of uncertain and remote effectiveness.

The elements which can be (and are) evidently relied on to secure the necessary safety are those which contribute to the mass and stiffness of the struc-

[2a] Numerals in parentheses, thus: (41), refer to corresponding items in the Bibliography (see Appendix of the paper), and at the end of discussion in this issue.

ture and, therefore, to its resistance against the building up of dynamic deformations. Principal among these elements are the following:

1. Dead Weight.—In the new design the dead weight appears to be at least 50% greater than that of the original bridge. It is now well recognized that even such a substantial addition to its weight would not have stabilized the old bridge, and that the addition of weight not otherwise required would have been wasteful. Nevertheless, in the new design the additional weight required, because of the greater width and traffic capacity of the bridge, adds substantially to its resistance to dynamic wind forces as they actually occur. In the case of the Geroge Washington Bridge (New York, N. Y.) it is practically weight alone that insures adequate stability.

2. Great Depth and Stiffness of the Stiffening Girders.—The author emphasizes, as an improvement in form, the substitution of trusses for the solid plate girders of the old design. He does not, however, bring out the important fact that these trusses, four times deeper than the girders of the old bridge, have been given an elastic stiffness vertically, as measured by their moment of inertia, thirty-seven times greater.

3. Location of the Deck and Its Resistance to Vertical Oscillations.—In the old design the floor stringers, the concrete slab, and the single lateral truss were located very close to the neutral axis of the stiffening girders, and, therefore, could have added only a negligible amount to the extremely low resistance of the latter against vertical oscillations. In the new design the stringers, the upper lateral system, and the deck are located practically in the plane of the top chords of the stiffening trusses and, in addition, a lateral system is provided in the plane of the bottom chords. Although expansion joints are probably to be provided in the deck, the frictional and elastic resistances of the stringers and laterals and also the damping effect of the concrete deck against the building up of vertical oscillations must be almost equivalent to the stiffening effect of the stiffening trusses. In other words, the resistance against vertical motions by reason of the deep trusses and the location of the deck and lateral systems in the plane of the chords of the trusses must be of the order of seventy times that of the old design.

4. Double Lateral System and Its Effect on the Resistance Against Torsional Motions.—As already mentioned, the old bridge had only a single lateral system approximately in the plane of the neutral axis of the stiffening girders. This, together with the relatively narrow width between girders, made the suspended structure extremely flexible in torsion and consequently low in its resistance against the building up of the fatal torsional motions. In the new design the width is more than 50% greater, and two lateral systems 33 ft apart are provided. This has the effect of increasing the torsional resistance of the suspended structure in the new design, compared to that of the old, in a ratio even greater than that of the respective resistances against vertical motions. It is safe to say, without exact analysis, that the new design possesses ample resistance against objectionable and dangerous torsional oscillations, whether the open slots in the deck are provided or not.

The effects of these various stiffening elements have as yet been insufficiently explored by model tests in wind tunnels. It is questionable whether they can

be reliably explored by such tests, because of the difficulty of adequately reproducing on a model in a wind tunnel the wind action on actual bridges and the internal damping and frictional resistances in the structure.

The author deprecates the value of experience gained from the behavior of actual suspension bridges on the grounds that it is allegedly not known what movements have occurred, or whether the bridges have been subjected to heavy wind storms and from what direction. Nevertheless, this is the most important and reliable source of information presently available to the profession for the purpose of deriving safe design criteria. There are or have been many bridges in existence which have experienced observable and more or less severe motions or failure. Their physical properties essential for an analysis of their resistance to wind are known. It is not necessary to have accurate measurements of the motions they have experienced as long as it is known under what resistance limitations of the structure failure or objectionable or dangerous motions have occurred. The author's argument, that it is not known whether existing bridges have been subjected to severest wind action, is unquestionably valid when applied to bridges of comparatively recent construction. There are, however, hundreds of suspension bridges which have existed, and whose behavior in wind has been known, for periods up to more than a century. Among them the great majority is known not to have developed objectionable motions. Of some twenty bridges that are known to have developed observable motions, several have suffered more or less serious damage to the structure and about half have failed.

It would be a serious reflection on the profession if it had to be admitted that adequate design limitations could not be derived from such a wealth of practical experience. The problem of deriving adequate and, within reasonable limits, economical design limitations for suspension bridges against wind action is, like many other design problems, not one which can be solved with mathematical accuracy. The influencing factors are too complex and too varied. Some of them are extremely difficult to determine. However, on the basis of the behavior of actual structures, it is as possible to derive design criteria within the same degree of safety and economy as it is to design individual members or composite structures subject to the action of static forces. Studies made by the writer and others show that design criteria can be developed, based on the weight and stiffness characteristics of the suspended structure, which are reasonably consistent with the behavior of the many suspension bridges whose behavior is known, notwithstanding the great variety of structural forms represented by them.

Until much more information is derived by research (such as experiments with large models in wind tunnels to determine the effect of form, and studies of the wind action on existing structures), the profession must rely on design features that assure adequate mass and adequate elastic and frictional resistances of the structure. This is, in the present state of knowledge, what had to be done in the case of the new Tacoma Narrows Bridge design.

CHARLES A. ELLIS,[3] M. ASCE.—The failure of the Tacoma Narrows Bridge made newspaper headlines for various reasons, one of which is particularly significant. It is common knowledge that medicine is by no means an exact science, and untimely deaths are commonplace. However, most everyone (including some engineering professors and authors) believes, or at least infers, that engineering is an exact science. It may be stated without fear of successful contradiction that no engineer ever correctly answered any engineering problem, unless his mistakes in computation exactly compensated for the errors inherent in all engineering theory. Consequently, when an engineering structure fails, the man has bitten the dog and headlines appear.

Jonathan Jones, M. ASCE, was a true prophet when he wrote (45) the following paragraph:

> "The design of other types of bridges is based upon their carrying certain loads at certain unit stresses; but a suspension bridge design cannot even be begun until there are stipulated, in addition to the loads and unit stresses, the criteria for maximum admissible flexibility. To a certain extent these criteria must be psychological, possibly as much so as mechanical. Whether such criteria of flexibility as have been and are being chosen are valid, and to what extent deviations therefrom would be regretted, is the most important remaining question in the field of suspension bridge design."

The maximum admissible flexibility was rapidly being approached in several suspension spans being built at about this time. It was reached and surpassed on the Tacoma bridge.

Just what are the criteria for maximum admissible flexibility? A comparison of the original Tacoma design with the writer's Golden Gate Bridge (San Francisco, Calif.) design may throw some light on this question.

The Golden Gate span is 50% longer; but the stiffening trusses, which are 90 ft apart, 25 ft deep, and have from 160 sq in. to 200 sq in. of silicon steel in each chord, have vertical and lateral moments of inertia many times larger than do the 8-ft plate girders, 39 ft apart, of the Tacoma bridge. The concrete deck located about halfway between the girder flanges gave little resistance to torsional movement. On the Golden Gate Bridge the sway bracing of diagonals from the bottom chords to the floor beams was intended to hold the top and bottom chords of each truss in a vertical plane, making it feasible to include the moment of inertia of the bottom chord with the lateral system of the top chord in resisting horizontal wind pressure. These diagonals act as a deterrent to torsional movements, although none were anticipated at the time the design was made. The maximum computed horizontal deflection at midspan due to a 30-lb wind load is about 23 ft, the maximum bending movement being at about each quarter point of the span (46).

Russell G. Cone, M. ASCE (37*a*), has reported that on February 9, 1938, when a wind of very high velocity was blowing through the Golden Gate Bridge and normal to the structure, with a force so great that he could not

[3] Evanston, Ill.

stand erect, the span was deflected an estimated 8 ft or 10 ft, and was held in this position. Vertical undulations were obvious, but no mention is made of torsional movements. Assuming that the wind velocity was 80 miles per hr, the pressure would be from around 20 lb to 25 lb per sq ft. The horizontal deflection was less than one half of the computed deflection for a 30-lb wind. This may be accounted for in part by the omission of the concrete deck and stringers in computing the lateral moment of inertia.

The ratio of cable sag to span length was considerably less in the Tacoma design than in the Golden Gate span. Any comparison should take cognizance of this fact because the smaller the sag, the greater is the stiffness of the system.

The computed maximum vertical deflections due to live load and temperature are nearly equal in the two bridges; so also are the computed horizontal deflections due to wind. Nevertheless, examination of the two designs and the performance of the two structures makes it difficult to escape the conclusion that the Golden Gate design is many times stiffer than the original Tacoma design. This poses a very important question. Can the criteria for maximum admissible flexibility be established, and, if so, what is the modus operandi for approaching the solution?

An important question, which unfortunately can never be answered, concerns the stay ropes installed on the Tacoma bridge, extending from the central cable band to the stiffening girder on each side. The structure was destroyed by torsional oscillations caused by the slipping of one cable band which threw the cables out of phase. Would the bridge still be standing had the cable band not slipped, or had the stay ropes never been installed?

F. H. FRANKLAND,[4] M. ASCE.—The problems described by Mr. Andrew applying to the design of the second Tacoma Narrows Bridge are naturally affected in their solution by considerable data recently developed relative to the effect of aerodynamic forces on suspension bridges.

The author states (under the heading, "Major Problems"),

> "* * * There is no actual, measured value for the damping in a suspension bridge. * * *
>
> "Because the damping capacity of the first bridge is unknown, the adopted design for the second bridge, or for that matter that for any suspension bridge, cannot be truly simulated in a model."

The Advisory Board on the Investigation of Suspension Bridges, in considering the effects of structural damping, realized that knowledge regarding frictional damping was so incomplete that an attempt must be made to approach the problem by rational methods in order to obtain an insight into the effects of dry friction acting between structural parts of suspension bridges. Consequently, Friedrich Bleich, M. ASCE, in connection with analytical mathematical studies for the Advisory Board, prepared a report on the effects of structural damping which covers comprehensive analytical studies of the aerodynamic behavior of suspension bridges. The results of these damping studies furnish analytical methods for arriving at the damping factor of any suspension bridge design.

[4] Cons. Engr., New York, N. Y.

From the aforementioned theoretical methods it is possible to assign a definite damping factor to both the original and the second design of the Tacoma Narrows Bridge if the theory is checked by tests. A series of damping tests on simple beams and trusses was made for the Advisory Board by the Division of Physical Research, United States Public Roads Administration, at the Arlington (Va.) laboratory. These tests disclosed that the internal damping capacity of modern steel structures (that is, damping capacity inherent in the elastic hysteresis of the material, resulting from the plastic yielding and friction in the riveted joints) is comparatively small. From these tests the conclusion could be drawn that a considerable part of the damping effect in steel bridges is the result of energy dissipation by internal and external dry friction.

Further tests at the Arlington laboratory are in progress for the purpose of examining the effect of friction between roadway stringers and floor beams and the effect of special friction devices on the damping capacity of suspension bridges. These tests, it is hoped, may afford an opportunity to check the reference made to theoretical studies.

The various reports on the collapse of the Tacoma Narrows Bridge failed to take into account the effects of the failure of the side span, tie down ropes installed about a month before the collapse on the initiative of the bridge authority. These side span ties consisted of $1\frac{9}{16}$-in. wire ropes fastened to the stiffening girders at points 300 ft from the anchorages, held down by concrete ground anchor blocks of 50 cu yd. When the first of these four ropes failed (November 7, 1940), a violent torsional motion was induced in the main span; with the vertical motions of the main cables thrown out of phase with one another, a severe stressing of the center diagonal ties resulted on the north side and a consequent slipping of the cable band and a rupture of the ties. A trigger action was thus created which immediately threw the main span roadway into violent torsion, one side of the span being freed from the restraint of the tie, while the other (south) side was still restrained by its diagonal ties. A careful study of the motion pictures taken during the collapse clearly shows this sequence of events.

The cable bands, to which the center diagonal ties were connected in the first bridge, were deficient in contact area with the cables, thus providing the possibility for an extremely high unit frictional stress, and a consequent slipping of the bands under the conditions existing at the time of failure. This deficiency is evidently recognized and provided for in the new design by incorporating three sets of diagonal ties in each truss at the center of the main span.

It is extremely doubtful whether the streamlined handrail will provide any beneficial effect. The vortex shedding from the comparatively large upper chord area dominates the airflow characteristic over the bridge deck and any effect from streamlining the handrails must be negligible, especially when the angle of attack of the wind is rarely normal to the plane of the roadway, and is widely variable.

Mr. Andrew does not refer to the cable sag at midspan used in the design of the second bridge. Is it to be assumed that the center sag of 232 ft (a sag-to-span length ratio of 1:12) used in the first bridge is to be maintained, thus

realizing a high stiffening factor from the main cables? The main cables of a suspension bridge are by far the most important stiffening elements in such a system, and, for flat sags, the cables exercise a dominant effect on resistance to torsion and in resisting vertical and lateral movements of the suspended structure. The principal contributing factor to the stiffness of the George Washington Bridge (New York, N. Y.) is not the weight of the suspended span, but the use of four cables, two of which are at present in excess, but which are necessary to carry a future increase in dead and live loads in the event that a lower deck is added to the structure.

Bibliography.—

(37) **"The Failure of Tacoma Narrows Bridge,"** a report to the Hon. John M. Carmody, Administrator, Federal Works Agency, Washington, D. C., March 28, 1941. (*a*) Appendix IX.

(41) **"Redesign of Tacoma Narrows Bridge,"** by Charles E. Andrew, *Engineering News-Record*, November 29, 1945, p. 64.

(45) *Transactions*, ASCE, Vol. 100, 1935, p. 1175.

(46) *Ibid.*, Vol. 98, 1933, p. 1104.

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DISCUSSIONS

ANALYSIS OF FRAMES WITH ELASTIC JOINTS

Discussion

BY ARTHUR B. MCGEE, AND C. W. JENKINS

ARTHUR B. MCGEE,[10] ESQ.—A practicable method for analyzing the effects of elastic, or yielding, joints has been presented by Mr. Stewart. However, the method of analysis as illustrated by the author in Fig. 2 is exceedingly "tricky" and subject to errors. Moreover, with transverse loads on all the columns and beams six additional traverse drawings would be needed to analyze the frame completely. For example, Fig. 2(*c*) is only one of four traverses required for joint C in order to distribute moments from all four directions.

Fortunately, a more general and easier procedure is available. A frame with rigid joints and with members having variable moments of inertia may be substituted for the original frame and solved by ordinary methods. The author mentions the possibility of such a frame. The actual shape of each member of such a frame is not important. The characteristics of the members can be determined easily and only one traverse is required for each joint; only three traverses, instead of eight, are required for the frame in Fig. 2 to handle any loading whatsoever. The author states that "* * * when the final closure of a traverse is at a joint having more than one elastic connection, the substitution of tapering members will prevent proper closure." The writer finds, however, that proper closure at such joints is accomplished by the method described hereinafter.

The procedure to be followed is to solve a substitute frame with members of variable I in the usual manner. The u-values and p-values (as defined by W. Ruppel,[11] M. ASCE) for a substitute member are

$$u = u_o \times \frac{\Delta_u}{\theta_u + \Delta_u} \qquad (1a)$$

and

$$p = p_o \times \frac{\theta_u + \Delta_u}{\Delta_u} \qquad (1b)$$

NOTE.—This paper by Ralph W. Stewart was published in December, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: May, 1948, by L. E. Grinter.

[10] Structural Engr., Bridge and Structural Design Div., City of Los Angeles, Los Angeles, Calif.

[11] *Transactions*, ASCE, Vol. 90, 1927, pp. 167–187.

Similarly,

$$v = v_o \times \frac{\Delta_v}{\theta_v + \Delta_v} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1c)$$

and

$$q = q_o \times \frac{\theta_v + \Delta_v}{\Delta_v} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1d)$$

in which the subscript o is used to designate the original member. Eqs. 1*a* and 1*c* are evident from Fig. 4. Eqs. 1*b* and 1*d* follow from

$$u\,p = u_o\,p_o = v_o\,q_o = v\,q \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1e)$$

Without knowing the shape of a member the fixed-end moments due to loads applied on the member cannot be found by ordinary methods. An end moment for a member with yielding joints is designated as a "yielding-end moment" (YEM) in this discussion. Yielding-end moments can be found readily and they are the same as the fixed-end moments for the substitute member. Several methods using the traverse are available for finding yielding-end moments when the fixed-end moments are known. End moment distribution may be used for this purpose also, as shown later. Following are general equations for finding yielding-end moments from fixed-end moments. The original member may be unsymmetrical and have different $\frac{\theta}{\Delta}$-ratios at the ends:

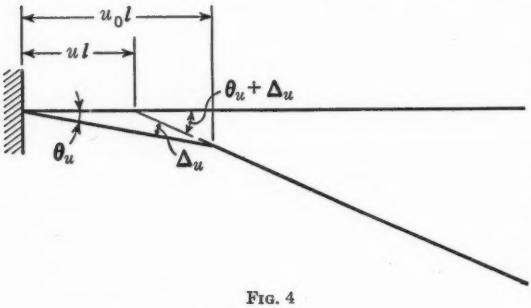

FIG. 4

$$\text{YEM}_{AB} = \frac{\text{FEM}_{AB}\,[n\,(1-u) + (1-u-v)] + \text{FEM}_{BA} \times u\,n}{m\,n + m\,(1-v) + n\,(1-u) + (1-u-v)} \ldots (2a)$$

$$\text{YEM}_{BA} = \frac{\text{FEM}_{BA}\,[m\,(1-v) + (1-u-v)] + \text{FEM}_{AB} \times v\,m}{m\,n + m\,(1-v) + n\,(1-u) + (1-u-v)} \ldots (2b)$$

in which the subscript AB and the quantities u and m refer to the left end of the member, and subscript BA and the quantities v and n refer to the right end of the member:

$$m = \frac{\theta_u}{\Delta_u} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2c)$$

and

$$n = \frac{\theta_v}{\Delta_v} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2d)$$

Eqs. 2*a* and 2*b* are not as formidable as they appear at first glance.

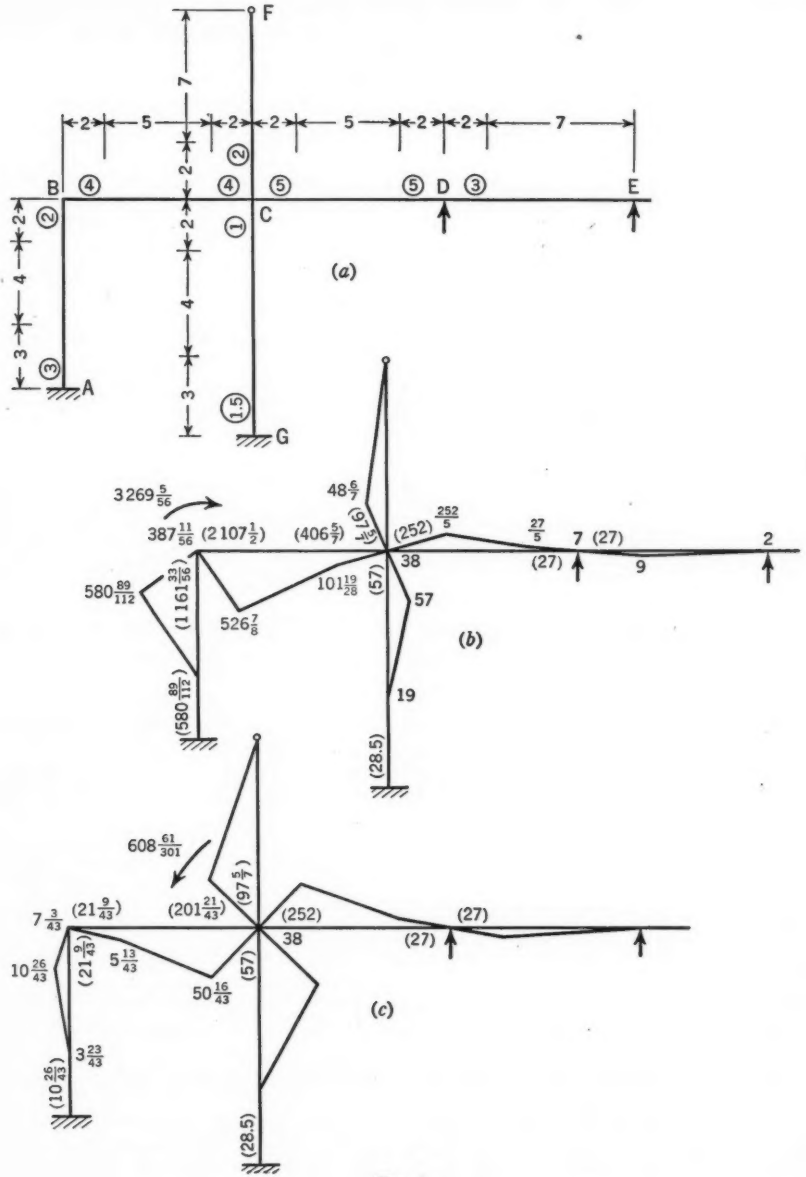

FIG. 5

If a member is of uniform I, with identical fixed-end moments and with identical $\frac{\theta}{\Delta}$-ratios at the ends, then both yielding-end moments are the same and

$$\text{YEM} = \frac{\text{FEM}}{\frac{\theta}{\Delta} + 1} \qquad (2e)$$

In the example worked by Mr. Stewart $\frac{\theta}{\Delta} = \frac{1}{2}$. Therefore, by Eq. 2*e*, YEM = $\frac{2}{3} \times$ FEM, and at ends where yielding occurs the u-values and v-values computed by Eqs. 1*a* and 1*c* equal $\frac{1}{3} \times \frac{2}{3} = 2/9$, and p-values and q-values by Eqs. 1*b* and 1*d* equal three halves of their former values. Traverse stiffnesses equal $\frac{E\,I}{p\,l}$ and $\frac{E\,I}{q\,l}$ at the two ends, respectively. Therefore, the stiffnesses of the substitute members will be two thirds of the stiffnesses in the original frame with the yielding joints, or, since stiffnesses need be proportional only, the stiffnesses at the yielding joints may be used unchanged—provided the two stiffnesses at the fixed column bases are increased by the factor 3/2.

Fig. 5(*a*) shows the substitute frame with stiffnesses encircled and u-values and v-values indicated. The traverses for externally applied moments at joints B and C are shown in Figs. 5(*b*) and 5(*c*), respectively. One additional traverse for a moment at joint D would permit the solution of any loading on any or all members of the frame.

The fixed-end moments for the uniform load of 12 kips per ft on member BC are 100 ft-kips, and the yielding-end moments, by Eq. 2*e*, are $\frac{2}{3} \times 100 = 66\frac{2}{3}$ ft-kips. Table 2 distributes the moments, giving totals that check the solution by Mr. Stewart.

TABLE 2.—DISTRIBUTION OF MOMENTS, IN KIP-FEET, CAUSED BY UNIFORM LOAD OF 12 KIP PER FT ON MEMBER BC OF SUBSTITUTE FRAME SHOWN IN FIG. 5

Description	End Moment at:									
	AB	BA	BC	CB	CF	CG	CD	DC	DE	GC
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Yielding-end moments.....			+66.67	−66.67						
$\frac{66.67}{3,269.09} \times$ Fig. 5(*b*).....	−11.84	−23.69	−42.98	−8.29	1.99	1.16	5.14	0.55	−0.55	0.58
$\frac{66.67}{608.20} \times$ Fig. 5(*c*).....	−1.16	−2.32	2.32	22.09	10.71	6.25	27.62	2.96	−2.96	3.12
Total.....	−13.00	−26.01	26.01	−52.87	12.70	7.41	32.76	3.51	−3.51	3.70

Of course, end moment distribution may also be used for solving the substitute frame. The carry-over factor is $\frac{u}{1-u}$ or $\frac{v}{1-v}$ and the end moment distribution stiffness is the basic stiffness, as used in the traverse, multiplied by $\frac{1-u}{1-u-v}$ or $\frac{1-v}{1-u-v}$, depending on the end of the member under consideration. Thus, carry-overs in both directions of members BC and CD equal 2/7, and column carry-overs to the fixed bases equal $\frac{1}{2}$. The stiffness at the top of column BA is $2 \times \frac{9-3}{9-2-3} = 3$; at the top of column CG, $1 \times \frac{9-3}{9-2-3}$

$= 1\frac{1}{2}$; at the bottom of column CF, $2 \times \frac{9-0}{9-2-0} = 2.571$; at the left end of beam DE, $3 \times \frac{9-0}{9-2-0} = 3.857$; at both ends of beam BC, $4 \times \frac{9-2}{9-2-2} = 5.6$; and at both ends of beam CD, $5 \times \frac{9-2}{9-2-2} = 7$. The distribution of the yielding-end moments by end moment distribution is carried out in convenient tabular form in Table 3, the results checking the other solutions.

TABLE 3.—DISTRIBUTION OF MOMENTS IN SUBSTITUTE FRAME IN FIG. 5 BY END MOMENT DISTRIBUTION

Description	Member							
	BA	BC	CB	CF	CG	CD	DC	DE
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Carry-over factor		↓ 2/7 ↓				↓ 2/7 ↓		
Relative stiffness	3	5.6	5.6	2.571	1.5	7	7	3.857
Distribution factor	0.349	0.651	0.336	0.154	0.090	0.420	0.645	0.355
Yielding-end moment	−23.27	+66.67 −43.40	−66.67 −12.40					
		+ 7.59	+26.58	+12.17	+7.12	+33.21	+9.50	
	− 2.65	− 4.94	− 1.41			− 1.75	−6.13	−3.37
		+ 0.30	+ 1.06	+ 0.49	+0.28	+ 1.33	+0.38	
	− 0.10	− 0.20	− 0.06			− 0.07	−0.25	−0.13
			+ 0.04	+ 0.02	+0.01	+ 0.05		
Total	−26.02	+26.02	−52.86	+12.68	+7.41	+32.77	+3.50	−3.50

$M_{AB} = \frac{1}{2} M_{BA} = -13.01$; $M_{GC} = \frac{1}{2} M_{CG} = +3.70$

Problems in partial fixation are in reality problems in elastic joints.

$$\frac{\theta_u}{\Delta_u} = \frac{(1-u-v)(1-r)}{r(1-v)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (3a)$$

or

$$r = \frac{\Delta_u (1-u-v)}{\theta_u (1-v) + \Delta_u (1-u-v)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (3b)$$

in which r is the fixation factor. Eq. 3*a* has been developed previously by the writer.[12] In the frame shown in Fig. 5, it was assumed that $\theta = \frac{\Delta}{2}$. Substituting this value of θ and inserting the values of u and v (namely, $\frac{1}{3}$) in Eq. 3*b* gives $r = 0.5$. Hence, the ends of member BC are 50% fixed. Placing the fixed-end moments of 100 at each end of the member and distributing by end

[12] *Transactions*, ASCE, Vol. 104, 1939, p. 549.

moment distribution gives

Carry-over = $\frac{1}{2}$

	B		C	
50%	50%	Distribution factor	50%	50%
	+100	FEM	−100	
−50	−50		+50	+50
	+25		−25	
−12.5	−12.5		+12.5	+12.5
	+6.25		−6.25	
....	....		....	....
....	....		....	....
....	....		....	....
....	....		....	....
$-66\frac{2}{3}$	$+66\frac{2}{3}$	YEM	$-66\frac{2}{3}$	$+66\frac{2}{3}$

This checks the yielding-end moments already found by Eq. 2*e*.

Fig. 6(*a*) shows a frame with one leg 25% fixed. From Mr. Ruppel's tables,[11] $u = 0.250$ and $p = 0.292$ at the lower end of the tapering leg; also,

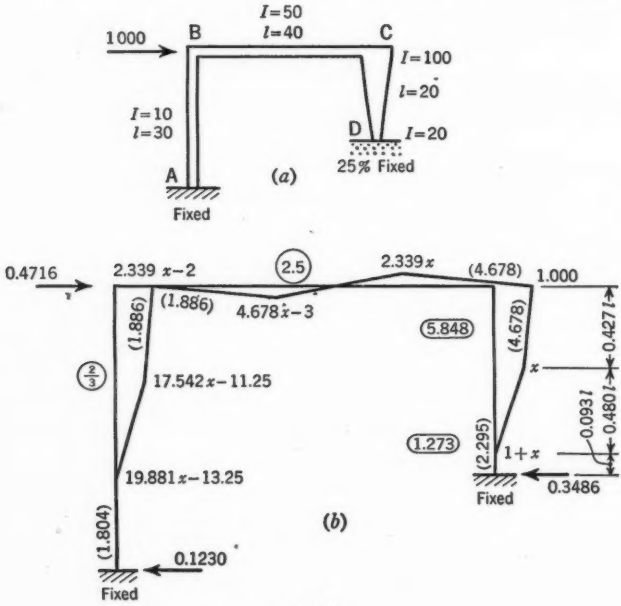

FIG. 6

$v = 0.427$ and $q = 0.171$ at the upper end of the leg. This frame has been analyzed before by another method.[13]

By Eq. 3*a*, $\frac{\theta_u}{\Delta_u} = \frac{(1 - 0.250 - 0.427)\,(1 - 0.25)}{0.25\,(1 - 0.427)} = 1.691$ and $\frac{\theta_u + \Delta_u}{\Delta_u} = 2.691$. Therefore, by Eq. 1*a*, $u = 0.250/2.691 = 0.093$, and, by Eq. 1*b*,

[13] *Transactions*, ASCE, Vol. 104, 1939, p. 550.

$p = 0.292 \times 2.691 = 0.786$ for the substitute leg, which will be fixed at the base. The relative stiffness at the bottom of the substitute leg DC is $\frac{\Delta}{M} = \frac{I}{p\,l} = \frac{20}{0.786 \times 20} = 1.273$, and at the top of the leg, $\frac{\Delta}{M} = \frac{20}{0.171 \times 20} = 5.848$.

In Fig. 6(*b*) the angles x and 1.000 are assumed, and the remaining angles are calculated by ordinary traverse procedure. Since the deflections in the legs are equal, the value of x must be 0.8026. A Δ-angle times the appurtenant stiffness gives the appurtenant end moment. The end moments for each member are shown in parentheses. The sum of the leg shears is the lateral force at the top of the frame. To find the effect of a lateral force of 1,000, multiply the moments shown by $\frac{1{,}000}{0.4716}$. This gives $M_A = 3{,}825$; $M_B = 3{,}999$; $M_C = 9{,}919$; and $M_D = 4{,}866$.

Frames with yielding joints can now be calculated in exactly the same manner as rigid joint frames with members of variable I.

C. W. JENKINS,[14] JUN. ASCE.—Use of the elastic curve traverse for the analysis of frames with elastic joints offers several profound advantages over methods previously presented. A study of the application of moment distribution,[15] slope deflection,[16] and the method of simultaneous balance of both moments and angles[17] will show each one to be more complex in the analysis of such frames for more than one condition of loading, or where the frame contains members having variable moments of inertia, or a combination of both. The method presented by Mr. Stewart is as basic and practical for these cases as it is for the analysis of a fully rigid frame. It offers the added advantages of an immediate self-check by Maxwell's law and a direct solution for the deflection of a frame.

A consideration of the elastic curve traverse will reveal its advantages. The traverse is a representation of the deflected frame, which expresses the joint rotation and the flexure angle due to moment at each end of a member.

The traverse is, therefore, a natural method of analysis of yielding joints. At the end of a member the joint yield is simply expressed as a percentage of the flexure angle caused by moment. It should be noted that expressing joint yield as a percentage of the flexure angle is a tool for analysis, and only that. For a given moment the flexure angle of the member depends on the elastic properties of the member, whereas the joint yield depends on the construction of the joint. The author's statements regarding the evaluation of the θ-ratio at each joint are of great importance.

As an illustration of the adaptability of this method to joint yield problems, the frame analyzed in the paper will be analyzed with another yielding joint added near the middle of a member. A semi-rigid splice has been inserted in column CG at a point 0.6 of the column height above G as shown in Fig. 7(*a*).

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[15] "Analysis of Building Frames with Semi-Rigid Connections," by Bruce Johnston and Edward H. Mount, *Transactions*, ASCE, Vol. 107, 1942, p. 997.

[16] *Ibid.*, p. 1001.

[17] "Elastic Properties of Riveted Connections," by J. Charles Rathbun, *ibid.*, Vol. 101, 1936, p. 561.

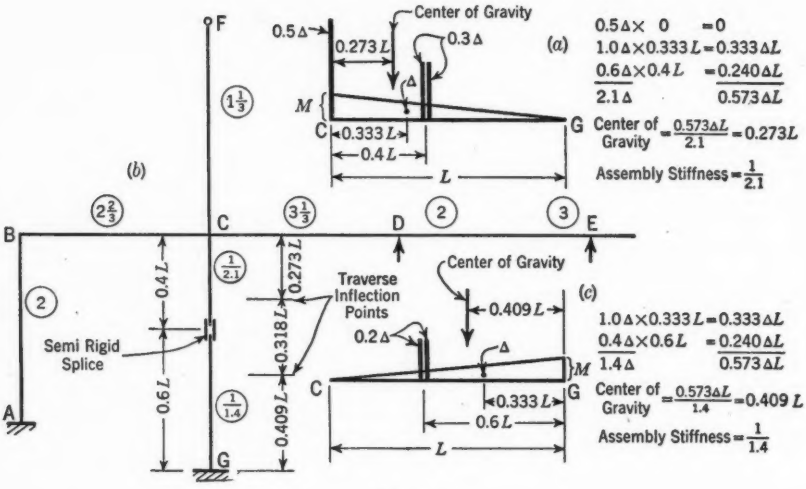
F
0.5Δ
Center of Gravity
0.273L
0.3Δ
(a)
0.5Δ× 0 =0
1.0Δ×0.333L=0.333ΔL
0.6Δ×0.4L =0.240ΔL
2.1Δ 0.573ΔL
1 1/3
M
C
G
0.333L
0.4L
L
Center of Gravity = 0.573ΔL/2.1 = 0.273L
Assembly Stiffness = 1/2.1
(b)
2 2/3
3 1/3
2
3
B
C
D
E
1/2.1
0.273L
0.4L
Traverse Inflection Points
Center of Gravity
(c)
0.409L
0.2Δ
Δ
M
1.0Δ×0.333L=0.333ΔL
0.4Δ×0.6L =0.240ΔL
1.4Δ 0.573ΔL
Semi Rigid Splice
0.318L
0.6L
1/1.4
0.409L
A
G
0.333L
0.6L
L
Center of Gravity = 0.573ΔL/1.4 = 0.409L
Assembly Stiffness = 1/1.4

FIG. 7

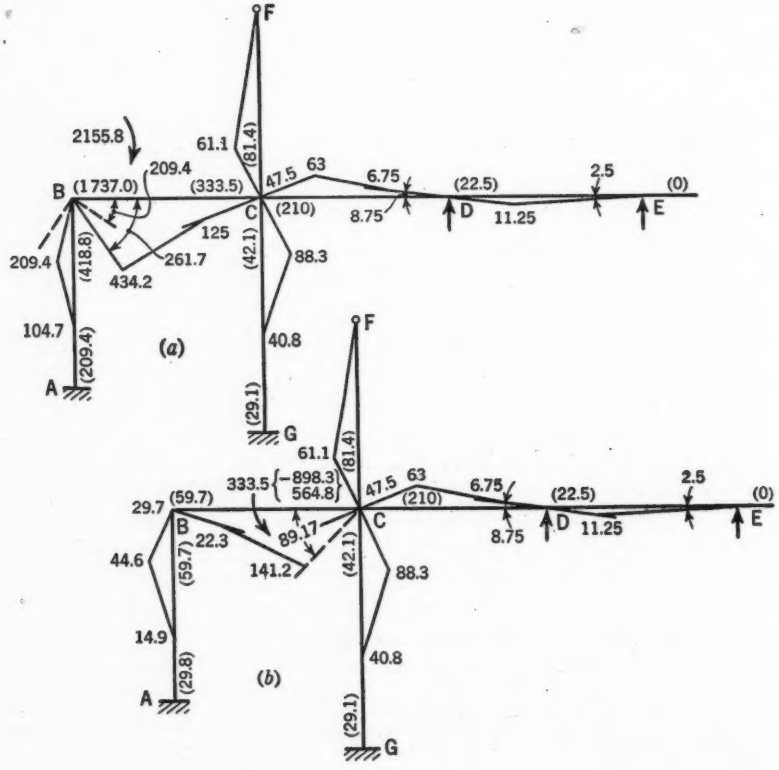
F
2155.8
61.1
(81.4)
209.4
B
(1737.0)
(333.5)
47.5
63
6.75
(22.5)
2.5
(0)
C
(210)
8.75
D
11.25
E
125
(42.1)
88.3
209.4
(418.8)
261.7
434.2
104.7
(209.4)
(a)
40.8
A
(29.1)
G
F
61.1
(81.4)
333.5
-898.3
564.8
47.5
63
6.75
(22.5)
2.5
(0)
29.7
(59.7)
B
22.3
89.1
C
(210)
8.75
D
11.25
E
44.6
(59.7)
141.2
(42.1)
88.3
14.9
(29.8)
(b)
40.8
A
(29.1)
G

FIG. 8

The conjugate beam theorem has been found to be exceedingly useful in determining flexure factors. The conjugate beam load for member CG is shown in Fig. 7(*b*). Since the area under the $\frac{M}{E\,I}$-diagram is Δ, Δ is used as the

TABLE 4.—MOMENTS, IN KIP-FEET, CAUSED BY UNIFORM LOAD OF 12 KIP PER FT ON MEMBER BC (FIG. 7)

Factor	From	M_A	M_{BC}	M_{CB}	M_{CF}	M_{CG}	M_{CD}	M_{DE}	M_E	M_G
(*a*) SEMI-RIGID CONNECTIONS WITH SEMI-RIGID SPLICE										
$\frac{100}{2{,}155.8}$	Fig. 8(*a*)	−9.7	19.4	−15.5	3.8	1.9	9.7	−1.0	0	1.4
$\frac{100}{898.3}$	Fig. 8(*b*)	−3.3	6.6	−37.1	9.0	4.7	23.4	−2.5	0	2.2
Total		−13.0	26.0	−52.6	12.8	6.6	33.1	−3.5	0	4.6
(*b*) SEMI-RIGID CONNECTIONS (FINAL MOMENTS ONLY)										
Total	Table 1(*a*)	−13.0	26.0	−52.9	12.7	7.4	32.7	−3.5	0	3.7
(*c*) RIGID CONNECTIONS (FINAL MOMENTS ONLY)										
Total	Table 1(*b*)	−21.1	42.2	−88.8	20.0	13.4	55.3	−10.4	0	6.7

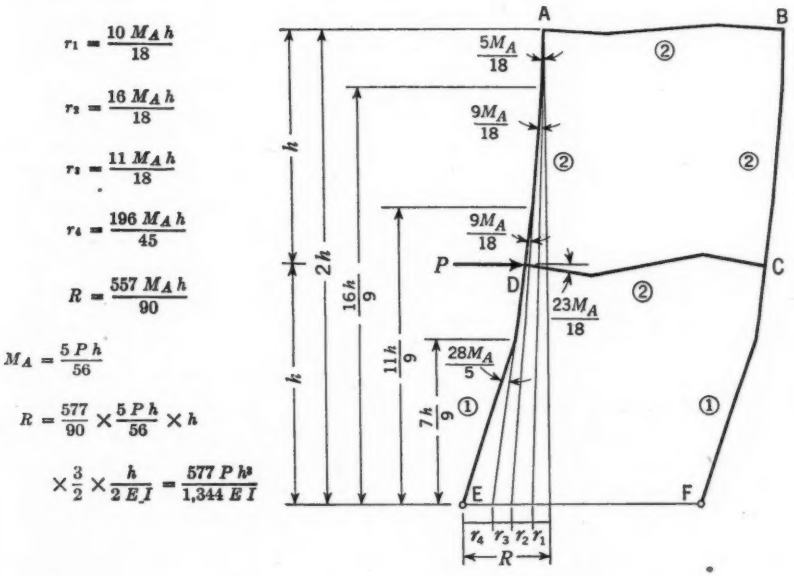

FIG. 9

load caused by flexure of the member. The load due to yield θ at joint C is $\frac{\Delta}{2}$. A semi-rigid splice would also have a yield of $\frac{\Delta}{2}$ on both ends of the splice plates if it were located at C. Inasmuch as a splice consists of two members

connected by splice plates, it has a total yield of $2 \times \frac{\Delta}{2} = \Delta$. However, the splice transmits only 0.6 M where it is located on column CG. Thus, the splice yield is 0.6 Δ, the load on the conjugate beam caused by the splice yield.

The ratio of the assembly stiffness to the member stiffness is now equal to $\frac{\Delta}{\phi}$, ϕ being the total load on the conjugate beam. The location of the ϕ-angle (which is the center of gravity of the conjugate beam load) is also easily found from Fig. 7(*b*). The assembly stiffness and ϕ-angle location for column GC are determined in a similar manner from Fig. 7(*c*).

The frame has been analyzed in Figs. 8(*a*) and 8(*b*) by the procedure outlined by Mr. Stewart. The resulting moments are given in Table 4 for comparison with those of the original frame.

The flexure traverse affords a direct determination of deflections. As an illustration of this fact, the two-story frame with semi-rigid joints used as an example in the paper (Fig. 3) is analyzed for deflection in Fig. 9. It should be noted that the true assembly stiffness, $\frac{2}{3} \times \frac{2EI}{h}$, must be used instead of the relative assembly stiffness to determine the actual deflection, R. All computations are shown in Fig. 9.

Mr. Stewart's paper marks definite progress toward a practical method of designing steel or timber frames as they actually occur in practice with connections of partial rigidity.

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DISCUSSIONS

LATERAL EARTH PRESSURES ON FLEXIBLE RETAINING WALLS A SYMPOSIUM

Discussion

BY RALPH B. PECK, HOWARD C. ROBERTS, ARMAND MAYER, S. PACKSHAW AND J. OWEN LAKE, AND JACOB FELD

RALPH B. PECK,[47] ASSOC. M. ASCE.—The following discussion is concerned only with those papers of the Symposium dealing with the tests at Princeton University (Princeton, N. J.). Professor Tschebotarioff has stated three objectives for the first series of tests. The writer believes that the first objective was successfully reached in large measure, and that the results of tests to determine the influence of sand backfill in reducing the lateral earth pressures of cohesive soils are of interest and value. The results of experiments to determine the lateral pressure exerted by fluid cohesive mixtures (the second objective) are of interest, but the interpretation of the results and their explanation seem to be unnecessarily complicated.

The studies of arching in sand backfills (the third objective) appear to have been planned and executed without adequate consideration of the vital factors involved. It is the writer's opinion that no valid conclusions concerning arching can be drawn from the results of the experiments as yet.

Distribution of the lateral pressure exerted by sand against a retaining structure depends primarily on the manner in which the retaining structure can deflect. Many statements by Professor Tschebotarioff indicate that he is conscious of this fact. Nevertheless, the writer believes that the deformation conditions associated with the experimental bulkhead were fundamentally so different from those associated with most real bulkheads that the results of the sand experiments are devoid of practical value in connection with bulkhead design.

It would seem advisable to precede an extensive series of experiments on bulkheads by field observations concerning the deflections of actual structures; this apparently has not been done. However, the type of deformation can be

NOTE.—This Symposium was published in January, 1948, *Proceedings*.

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inferred from a general knowledge of the stress-deformation characteristics of somewhat similar structures. Commonly, a bulkhead is made by driving sheet piles in shallow water. The anchorage is constructed, and the sheet piles are connected to it by tie rods. Up to this point no significant deformations of the soil mass are experienced.

The next step is likely to be the backfilling of the sheet piles. While this occurs, the anchor rods are stressed, whereupon they stretch and also tend to pull the anchorage system toward the channel. If the anchorage system is properly designed, these movements are relatively small, on the order of 1 in. or 2 in. At the same time, however, the weight of the backfill causes shearing stresses in the soil in which the lower ends of the sheet piling are buried. Under the influence of these stresses, together with the lateral pressure of the fill against the lower part of the sheeting, the sheet piles below the anchor also tend to move toward the channel. This movement is likely to be at least as large as that at anchor level.

The final stage of construction involves dredging the channel to the desired depth. During this stage the middle part of the sheet piles, and even their lower ends, are likely to move out energetically unless the underlying material is very stiff. This outward movement may be several times as great as that at anchor level.

Since bulkheads are commonly built where the subsoil conditions are relatively poor, it is the rule rather than the exception that the sheet piles should move outward near the dredge line, a distance considerably more than at anchor level. This constitutes the most important deformation condition. Nevertheless, the apparatus at Princeton was so constructed that movements of this type could not be simulated. Although the support at level B could be moved outward by some distance (the papers do not indicate how much), the support at C was not capable of translation. This fact must have appreciably restricted the outward movements possible in the vicinity of the bottom of the bulkhead. Even in spite of this restriction, the distribution of pressures inferred from the measurements indicated a moderate degree of arching because the center of pressure of the curves shown in Fig. 23 is at approximately 0.38 of the distance from the dredge line to the top of the bulkhead.

Apparently the restrictions on outward movement were recognized and the third series of tests was performed to simulate more nearly the conditions in the field. The results of bending moment and deflection observations for one test are shown in Fig. 27; computed lateral pressures are shown in Fig. 28. These pressures, however, were obtained by a method so crude as to eliminate them from serious consideration at the present time. The process of double differentiation to obtain pressure from measured bending moments is subject to very large errors, especially if the distribution of pressure is as irregular as that shown in Fig. 28.

The procedure described by Professor Tschebotarioff was reasonably accurate in connection with the bulkhead arrangement for the first tests because the reactions were measured and the laws of statics could be applied to provide a check. However, even under these relatively favorable conditions, errors in pressure of as much as 20% are indicated in the calibration curve (Fig. 12)

for the bulkhead acted upon by water. The writer would have no confidence whatsoever in the results of any pressure distribution curves obtained by a process of double differentiation without at least a method for checking the computed pressures by the measurement of reactions.

An additional factor tending to reduce any tendency for arching in the experiments pertaining to Fig. 28 is the lack of backfill at and above anchor level. Hence, the writer does not believe that either the accuracy of the computations or the test arrangements justify Professor Tschebotarioff's statements that arching is unlikely to occur in sand behind real bulkheads. On the other hand, the writer is inclined to be somewhat more impressed with the observations made by Danish engineers of some bulkheads that had stood up, whereas, according to conventional ideas, they should have collapsed. Where the results of field observations and experience with full-sized structures appear to be in contradiction to those of somewhat questionable experiments, the writer would consider it unwise to place much reliance upon the experiments.

Concerning the deformation conditions—the writer would like to point out that the bending of the sheet piles is likely to be a matter of small consequence in the redistribution of pressure. Probably of far greater importance is the movement of the anchor and the outward movement of the sheeting near the dredge line. As nearly as can be judged, the experimental arrangement for the third series of tests permits appreciable outward movement of the sheet piles at the dredge line. The actual stiffness of the piles is not stated. It is of interest to note that the deflection gages, provided to observe the movements of the buried part of these piles, could not extend far enough to measure the actual movements. This suggests the extent to which such movements were underestimated and serves to explain some of the shortcomings of the original apparatus.

Furthermore, as the apparatus now exists there is no satisfactory way of determining the lateral pressures. It would seem necessary to find some method of determining the actual shears in the piling near the dredge line. A similar necessity with measurements in open cuts led to the expedient of cutting the piling at the bottom of the cuts and of measuring the reactions necessary to hold the piling in its existing position. Such measurements are not convenient or highly accurate, but they are far more accurate than the results of estimates based on the measurements of bending moments.

In Part II, Section B, Professor Tschebotarioff discusses at some length various methods for computing the earth pressure of soft cohesive soils against the model bulkhead. Much of the discussion is devoted to classical earth pressure theories and to the results of measurements against the bracing of open cuts in clay. The writer believes that this discussion is largely irrelevant.

All theories of this type are based on the assumption that the backfill has reached a state of failure either throughout a considerable region, or at least in the neighborhood of some surface of sliding. These conditions may be realized even in soft natural clay deposits when an open cut is excavated because a moderate deformation is likely to be sufficient to develop the shearing resistance of the soil. Furthermore, the deformations of a natural clay stratum

during excavation are fully effective in producing shearing strains and developing shearing resistance in the soil mass.

In cases of bulkheads backfilled with almost fluid materials, this condition may not be realized. At the time of placement, the shearing resistance of the backfill is largely of a viscous nature. It seems unlikely that permanent shearing stresses can exist during the first days after construction. However, under the weight and lateral pressure of the backfill, large movements of the sheet piles can (and do) occur. Because of the viscous nature of the backfill, the pressures exerted by the soil mass remain largely hydrostatic and a definite shearing resistance along a potential surface of sliding cannot be developed.

Hence, at first the lateral pressures are likely to be almost equal to those of a fluid. As time passes, the backfill material may either consolidate or harden. If hardening occurs, it is likely to be associated with a shrinkage of the clay and possibly a backward movement of the bulkhead. Consolidation would occur not only vertically, but also laterally, under the influence of the small but nevertheless real forces of restitution exerted by the deflected bulkhead. These forces would lead to the observed backward movement of the model bulkhead.

The final condition would correspond to what the author terms the consolidated equilibrium condition. Since further outward deformations would be necessary to establish a state of failure in any region of the backfill, it seems obvious that the lateral pressures observed against the model bulkhead could not be computed successfully by any earth pressure theory based on the shearing resistance of the soil. Hence, the writer would consider the behavior of the clay backfills quite normal and sees no reason for attempting to invoke earth pressure theories. There does not appear to be any valid analogy between a bulkhead retaining a hydraulically placed backfill of clay-like material and an open cut in a natural soft clay deposit, and there seems to be no point in discussing design procedures developed for open cuts.

In conclusion, the writer would state that the Princeton tests represented a very ambitious program. They have undoubtedly served a very valuable practical purpose in providing information about sand dikes during the national emergency when such information was highly important. On the other hand, the writer believes that the so-called fundamental information derived thus far from the tests is disappointing and, in the case of sand backfills, misleading. It would be unfortunate if more satisfactory information were not obtained by modifying the test procedure so as to take proper account of deformation conditions, and to provide reliable means for estimating the distribution of lateral pressure.

HOWARD C. ROBERTS,[48] ESQ.—In one of the early paragraphs Professor Tschebotarioff describes the paper as a progress report. It is in this vein that the writer wishes to offer a number of comments and suggestions.

The test program described is an ambitious one, and will undoubtedly serve as an example to other investigators, and for other installations. It is apparent because of this, and also because of the small amount of such information

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available in the literature, that any additions or emendations which will serve to inform the reader better will be worthwhile.

One most serious criticism to be offered by the writer has to do with the manner in which the paper is written. The experimental techniques used in this project are certainly as interesting as the reported conclusions (at least to the writer). To a person expecting soon to engage in similar test work, they might well be far more interesting; and to a person interested only in the quality and nature of the conclusions, a detailed account of techniques employed is useful in providing criteria for judging the results. The descriptions of the apparatus and the techniques in this paper are not sufficiently detailed, nor are they specific enough in the details which are presented.

Under the heading, "Part I. The Program of First Test Series" of Professor Tschebotarioff's paper, there are two points where additional information is desirable. First, what were the experimental circumstances which caused or contributed to the scatter in the bending-strain values for the bulkhead? The wire type resistance strain gages (apparently) used are subject to many sources of error, but most of these errors are systematic—not random. The scattering of points suggests mechanical difficulties—varying restraints or the like—but the description does not contain information enabling the reader to form an opinion. Second, the Carlson pressure cells used for measurement of end reactions are stated to have caused trouble by clogging. This implies (and it is a circumstance to be expected of the usual form of Carlson cell) that the movement of the cell diaphragm was large enough not to be negligible. What, exactly, are the conditions of this measurement, and would not a null method pressure measuring device be preferable?

Since the data obtained from these gages were used in a computation involving double differentiation, small errors in the observed data might easily lead to far larger ones in the final result. It is the writer's opinion that on the basis of the published information this method should be considered subject to question.

Under the heading, "Part I. The Lateral Earth Pressure Meter" in the paper by Messrs. Ward, Bayliss, and Brown, there is described a device called a "lateral earth pressure meter." In this device, the lateral pressure resulting from a vertical load applied to a confined soil mass is measured in terms of the deflections of a series of split rings forming the walls of the device. The condition of the sample is described as one of "almost perfectly rigid support." Whereas it is probably true that for most samples the condition could be considered as equivalent to a perfectly rigid support, it has the appearance of a hybrid—representing an approximation the exact degree of which is not known. There is too little restraint to be called complete, but far too great to be comparable to that near most sheet piling bulkheads. In this case, too, it would seem that a null method is much to be preferred. Such a system can be set up with little more difficulty than that herein described (this statement is based on the writer's own experience), and it may be worth doing if for no better reason than the effect on the engineer's peace of mind.

In "Part II. Combined Active and Passive Earth Pressure Tests," there is a brief description of a transformer type deflection gage (a variable mutual in-

ductance gage) and its installation. Gages of this kind have had wide applications for many years, yet are virtually unknown in general engineering circles. A detailed description with at least approximate specifications for construction should be included. In this connection, it may be as well to state that the writer's experience includes work with magnetic gages which required heating before use; but as far as his memory serves, the effect was always traceable to the selection of an unsuitable magnetic core material or an improper design of the core.

The writer wishes finally to point out that he desires to offer constructive criticism of a paper describing a project of great promise.

ARMAND MAYER,[49] M. ASCE.—It is most gratifying to see that, after having computed the pressure on retaining walls or bulkheads so many times, the validity of the assumptions forming the basis of these calculations can be checked. The papers of Karl Terzaghi,[50] Hon. M. ASCE, in 1936 had already made clear the mechanism of the pressure on a retaining wall, and had shown that all the classical methods of computation supposed a relatively important displacement of the wall, a condition not necessarily reached with modern construction methods.

Professor Tschebotarioff based his experiments on somewhat different assumptions—a flexible bulkhead, rigidly fixed at its base and at a definite level by an anchorage. However, like Professor Terzaghi, he shows that the classical assumptions would be verified only if there were a shift of the bulkhead (undesirable by modern standards), so that the calculation should be made with a distribution of pressures very different from that generally accepted. Therefore, he should be thanked, but also encouraged to take up or to continue the tests so as to give, for all cases, the basis of computation that will lead to practical results. It would be regretted that, after having brought together the setup seen in Princeton N. J., by the writer, Professor Tschebotarioff would limit himself to the study of anchored bulkheads, in which the flexibility of the bulkheads brings in a factor almost impossible to take into account.

The writer would like to see model tests on the whole question of retaining walls—that is, measurement of the pressures at different levels for different natures of soil, with walls first rigid, then tilting within very small limits, as in actual construction. This should lead to experimental results that would replace the tables of Jean Résal[51] or Hans Krey,[52] of which the limits of validity are well known, but whose use is continued because others are not available.

This would certainly be a very extensive program; but Professor Tschebotarioff has the tools at hand to accomplish it. He is to be congratulated for what he has already done and should be encouraged to continue in the same line and to give the profession these needed additional data.

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[49] Inspecteur Général des Mines, Ministère de l'Industrie et du Commerce, Paris, France.

[50] "Large Retaining-Wall Tests," by Karl Terzaghi, *Engineering News-Record*, February 1, 1934, pp. 136–140.

[51] "Poussée des Terres," by Jean Résal, C. Béranger, Paris, 1910.

[52] "Erddruck, Erdwiderstand, und tragfähigkeit des baugrundes," by Hans Krey, W. Ernst und Sohn. 1st Ed., Berlin, 1912.

S. PACKSHAW,[53] M. ASCE, and J. OWEN LAKE,[54] Assoc. M. ASCE.—The tests carried out by Professor Tschebotarioff and his assistants are the most elaborate that have ever been undertaken. His objective is finding the answer to problems that have an important bearing on the design of sheet pile retaining walls, and he has therefore rendered a service to the engineering profession. The results, however, have shown very little evidence of arching in the sand backfill. This finding is so much at variance with accepted theories, and also with practical tests, such as those of J. P. R. N. Stroyer,[21,23] that one feels compelled to question whether the conditions in the model correspond with those that develop in the case of full-scale retaining walls backed by cohesionless material.

The design of the apparatus is not likely to have a great influence on the results of testing very soft clay backfills with a consistency approaching the liquid limit. For firmer cohesive materials, however, it seems doubtful whether the author's deductions about the relation of lateral pressure to shearing strength are justified.

In Part II, Section B (under the heading, "(2) 'At Rest' or 'Consolidated Equilibrium' Lateral Pressures"), it is stated that:

> "According to one current popular concept,[11,12] outward motions of more than 5% of the height of the bulkhead are needed to mobilize fully the shearing strength of a clay soil which is effective in reducing lateral pressures."

Professor Tschebotarioff disagrees with this assumption and states that the tests at Princeton University (Princeton, N. J.) show that such a concept applies neither to plastic backfills behind flexible bulkheads nor to the "consolidated equilibrium" pressures. The writers have always assumed, however, that the value given by Karl Terzaghi, Hon. M. ASCE, of a 5% movement is intended to indicate the approximate magnitude of the yield that must occur in a bulkhead or in the timbering of a trench in order to reduce the "at rest" pressures in a cohesive backing to the "active pressure," assuming that no change in water content takes place. The hypothesis that a reduction of lateral pressure takes place because of forward movement of the wall is based on a completely separate phenomenon from the one in which the reduction in lateral pressure is due to the consolidation of a plastic backfill under its own weight.

It is suggested that the reason why no apparent reduction in lateral pressure with increased outward motion of the bulkhead was observed can be attributed to the movement being limited to only 1.5%. Even more important is the fact that the clay possessed a very low shearing strength. Although the forward movements of a retaining wall backed by very soft clay do not result in

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[54] Civ. Engr., The British Steel Piling Co., Ltd., London, England.

[21] "Earth-Pressure on Flexible Walls," by Jens Peter Rudolf Nielsen Stroyer, *Journal*, Inst. C. E., London, Vol. 1, 1935–1936, pp. 94–139 and 550–557.

[23] "Earth-Pressure on Flexible Walls," by Rudolf Nielsen Stroyer, *Minutes of Proceedings*, Inst. C. E., London, Vol. 226, 1929, pp. 116–134.

[11] "A Fundamental Fallacy in Earth Pressure Computations," by Karl Terzaghi, *Journal*, Boston Soc. of Civ. Engrs., April, 1936, p. 87.

[12] "Theoretical Soil Mechanics," by Karl Terzaghi, John Wiley & Sons., Inc., New York, N. Y., 1943, p. 96.

an appreciable reduction of lateral pressure, it is erroneous to reason that similar results are to be expected with cohesive backfills of higher shear strength; in other words, in the initial stages of wall movement the behavior of a very soft clay backfill, as compared with that of a stiff clay, will more nearly exhibit the characteristics of an ideal soil whose shearing resistance is independent of the degree of deformation. Furthermore, as the shearing resistance of a very soft clay is very small, the deformation resulting from wall movement is almost in the form of viscous flow, rather than plastic flow, which occurs with stiff clays.

It should be pointed out that very deep excavations have been made through clay within sheet pile cofferdams which have been braced internally to resist pressures computed on the assumption that the active pressure approximates that shown in Fig. 18(*a*). These designs have been justified by subsequent performance. Furthermore, the measurements made by Ralph B. Peck,[17] Assoc. M. ASCE, on the Chicago (Ill.) subway were in reasonable agreement with the theoretical total pressure based on the conception indicated by Fig. 18(*a*). Professor Tschebotarioff's statement that: "The concept illustrated by Fig. 18(*a*) * * * has not been definitely limited to such conditions [stiffly plastic clays]," is therefore justified.

The angle ϕ_a as given in Fig. 11 is stated to be almost identical for clay and sand-clay mixtures. Such a result is only to be expected, as both materials were initially of an almost fluid consistency. The coarser grains of the sand-clay mixture would be in a loose and random distribution, whereas the clay fraction would be in a honeycombed, and therefore extremely compressible, state. This is confirmed by the statement in Part I (under the heading, "D. Soils Used for the Tests") that:

> "* * * the unconfined compressive strength and the water contents of both types of backfill were found to have an almost constant value throughout the entire depth."

Under such conditions, the shearing characteristic of these materials is likely to be almost identical during the early stages of consolidation, as the shear strength is then almost entirely dependent on the properties of the clay matrix. Therefore, it was not surprising that the lateral pressures exerted by these two materials were very similar.

Referring again to the previously quoted paragraph from Part II, Section B, the phenomena of decreased bending strains, deflections, and horizontal reaction pressures observed during the natural process of consolidation of the fluid backfill can be explained by the action of excess hydrostatic pressures which occur during the consolidation process. Immediately after the clay backfill is deposited the "effective stress" is equal to zero because the process of consolidation has not commenced. During the subsequent process of consolidation the excess hydrostatic head gradually reduces to zero, and the "effective stress" correspondingly increases from zero to its final "consolidated equilibrium value." Thus, there is a noticeable reduction of total lateral pressure which was observed by the investigators.

[17] "Earth-Pressure Measurements in Open Cuts, Chicago (Ill.) Subway," by Ralph B. Peck, in "Earth Pressure and Shearing Resistance of Plastic Clay: A Symposium," *Transactions*, ASCE, Vol. 108, 1943, pp. 1008–1036.

The expression which Professor Tschebotarioff has developed to relate the laboratory test results with the measured values of lateral pressure can be more simply written as

$$p_h = \gamma h - 2 c_e \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (53)$$

From Eq. 53 it can now be seen that, if ϕ is assumed to be zero and c_e is the apparent cohesion obtained due to the overburden pressure, there is fundamentally no difference between Eq. 7 and the usual equation for the pressure of cohesive soils. For instance, the value of c_e for the fluid clay backfill at the surface is essentially zero in Fig. 18(*a*) as well as in Fig. 18(*b*).

As regards the phenomena of arching in sands (Part II, Section C), the sand was deposited through water, and under such conditions it is logical to expect only small percentage reductions in total pressure, because the water pressure had already caused the largest proportion of the deflection. The addition of the sand would scarcely cause an increase in deflection amounting to more than a third of that caused by the water pressure. Consequently, the additional deflection induced by the sand fill may not always be sufficient to reduce the lateral pressures appreciably by arch action.

Referring to Fig. 21—which shows six possible limit cases of load distribution, the total load remaining the same—the author infers that redistribution cannot possibly be arranged to yield a reduction greater than 50%. A reasonable diagram can be drawn, however, which gives an even greater reduction, especially if the anchor is located slightly below the top of the wall.

In Part II, Section D, Professor Tschebotarioff states: "* * * that the dominant factor in any actually possible reduction of pressures may be the influence of an underlying rigid boundary * * *." The writers agree completely that such an influence is always present, but they feel that the author has been misled into thinking that arching cannot occur to any appreciable extent simply because the Princeton tests gave little or no indication of this phenomenon. It is the writers' belief that the absence of any noticeable arching was due to those conditions in the model not analogous to conditions usually present in a full-scale bulkhead in the field. For instance, the 4-ft-wide sloping steel plate immediately behind the bulkhead at the level of support B may have caused misleading results. The presence of this plate forced the slip plane to start from the level of support B, whereas in an actual retaining wall the slip plane would probably extend somewhat deeper. Furthermore, the tests showed that, if the sloping plate were released, arching would be immediately destroyed. This is only to be expected as a state of flux is then created which destroys the arch action; unless the plate was very rigid, it might possibly have deflected sufficiently under the weight of the backfill to interfere with arching and thus to cause the lack of stability which was observed in the test.

Another factor, considerably influencing the results, is that the sand used in the Princeton test was a particularly uniform, medium sand with a uniformity coefficient of about 2 and a voids ratio during testing of about $e = 0.8$. A material possessing these characteristics is likely to prove unstable when subjected to changes in stress conditions; the changes would inevitably occur

during flexing of the bulkhead and it may be that this factor explains, at least in part, the absence of any noticeable reduction in lateral pressure due to arch action. It is to be noted that, in the comprehensive range of tests conducted by Mr. Stroyer, the sand of the Thames River in England was found on analysis to be well graded and was considered to be a fairly representative sample of the material commonly used for backfill of retaining walls. Fig. 66 shows the grain-size distribution curve of the sand used in Mr. Stroyer's tests.

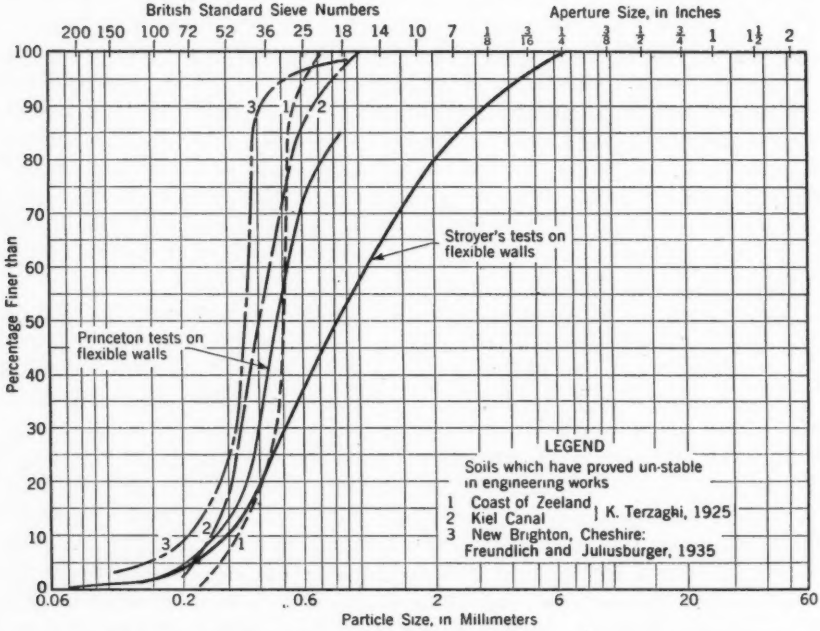
British Standard Sieve Numbers
200 150 100 72 52 36 25 18 14 10 7
Aperture Size, in Inches
1/8 3/16 1/4 3/8 1/2 3/4 1 1 1/2 2
Percentage Finer than
100 90 80 70 60 50 40 30 20 10 0
Stroyer's tests on flexible walls
Princeton tests on flexible walls
LEGEND
Soils which have proved un-stable in engineering works
1 Coast of Zeeland
2 Kiel Canal
K. Terzaghi, 1925
3 New Brighton, Cheshire: Freundlich and Juliusburger, 1935
0.06 0.2 0.6 2 6 20 60
Particle Size, in Millimeters
Fine Medium Coarse Fine Coarse
SAND GRAVEL

FIG. 66.—COMPARATIVE GRAIN-SIZE DISTRIBUTION CURVES

Fig. 66 also gives the distribution curves for three types of sand which, as quoted by R. Glossop and A. W. Skempton,[55] have proved unstable because of their uniformity and particle size.

The results of the Princeton tests do not appear, therefore, to prove that no arching takes place behind flexible walls. The writers hope, however, that further tests will be undertaken by Professor Tschebotarioff, and that the apparatus will be modified in so far as it is necessary to reproduce field conditions.

JACOB FELD,[56] M. ASCE.—No amount of theoretical analysis based on arbitrary assumptions can produce even a small part of the advance in knowledge resulting from such experimental work as is reported by Professor Tschebo-

[55] "Particle-Size in Silts and Sands," by R. Glossop and A. W. Skempton, *Journal*, Inst. C. E., London, Vol. 25–26, 1945–1946, p. 81.

[56] Cons. Engr., New York, N. Y.

tarioff and his associates. The writer can appreciate the extent of time and energy expended in this research problem, based on his own experience in the program on the determination of lateral earth pressure on rigid walls at the University of Cincinnati (Cincinnati, Ohio).[57] In addition, Professor Tschebotarioff had a time schedule within which usable results were required, and he must be complimented not only on the performance of the task assigned, but also for the concentration necessary to design, build, and calibrate the apparatus and also to perform the tests in such a short period of time.

The apparatus used in the first series of tests (Fig. 5) has some objections, from the point of view of similarity to the actual conditions encountered in an anchored sheet pile bulkhead. These objections are chiefly concerned with the existence of a pin-connected reaction, *C*, at the bottom of the sheeting, which cannot be expected to simulate an actual condition of embedded sheet piles. In the revised apparatus, used in the later series of tests, these objections are eliminated and the results must therefore be taken with greater confidence.

Incidentally, a similar division of the bulkhead into three sections, with pressures measured on only the center section, was used by R. O. Comer in determining the clay soil pressure for the design of the locks for the Inner Harbor Navigation Canal at New Orleans (La) in 1921. A summary of the results and a general description of the apparatus were included in a discussion by the late Henry Goldmark,[58] M. ASCE. The pressures were determined by successive differentiation of the measured deflections of a steel plate 8 in. wide and 6 ft high. The adjacent strips were free to move and deflect relative to the test area and to the sides of the bin. In general, Mr. Comer's results indicate a liquid pressure for freshly placed clay fills, reducing considerably with age, as the clay consolidated—sometimes as much as 18 in. in the height of 6 ft.

Supporting the test plate at three points introduces indeterminate conditions so that the results do not give sufficient data to solve all the equations and obtain the amount, direction, and distribution of the soil pressures. The measuring devices used are the most accurate apparatus yet employed for these purposes and require infinitesimal movements or strains to determine the stresses, from which the loads are computed. However, since the values of the individual reactions, *A*, *B*, and *C*, are dependent upon the relative displacement of the support, a questionable item is introduced. This is especially true because the direction of the reaction at the base of the sheet pile is opposite to *A* and *B*. The apparatus for the first series of tests does not permit the sheeting to take the natural shape, in the lower region, with a resultant effect on the reaction. Because of the pin-connected base, all the bending strain curves (Figs. 12, 15, 17, and 24) should be carried to a zero value at that level.

It is the influence of the *C*-reaction condition, probably more so than that of the lower boundary, as the author states, which has affected the values obtained in the lower regions. In the presentation of so much factual data, compressed by necessity into the permitted length of a Symposium paper, it might have been wise to omit the hypothetical pressure distribution curves in Figs. 19

[57] *Transactions*, ASCE, Vol. 86, 1923, p. 1448.
[58] *Ibid.*, p. 1555.

and 23. Although they are both designated by a question mark, there is no experimental result in the author's tests to prove the shape or location of the curves.

The results of these tests must be used to their full value for the design of sheet piles. However, it should be pointed out that an investigation of a sheet pile bulkhead as a structural unit is also necessary. The writer had occasion recently to show that the lateral movement of an anchored steel sheet pile bulkhead was easily explained by considering the entire structure as a unit. Although the sheet piling by itself had sufficient strength and did not seriously deflect, the total pressure on the structures, consisting of sheet piles and anchorages, was not balanced by a sufficiently large passive soil resistance.

One of the important conclusions of this paper is the proof that the Danish assumption of pressure distribution on sheet piling is not reliable. Another conclusion of great value is the fact that a relatively thin layer of sand against the sheeting does not reduce the liquid pressure of clay fills. This finding should affect the usual reliance on a bagged layer of gravel against a wall as a guarantee that the wall need not be designed to resist liquid pressure, when plastic fills are used. It may be surprising, however, to note that a sand fill equal to one half of the bulkhead height, only partly nullified the fluid clay backfill pressure. This result is equivalent to the findings from other research projects that surcharges beyond the slip or fracture surface do affect the lateral pressure of the fills.

The final report of the later test series should furnish sufficient data for the complete solution of the sheet pile problem. It would be easier to analyze the results of the second and third series, if the first tests were made in more homogeneous soils. The embedment of the sheeting in clay with a backfill of sand introduces possible effects on the deflections beyond those that would be found if the entire soil body was either sand or clay. The explanation of the inconsistent results shown in Fig. 28 for stages 4*a* and 6 may be found in this factor of the test program. Both of these stages of the test series are at sudden changes of the sand backfill, each of which must seriously affect the conditions of the sheeting embedded in the underlying clay. Further tests will undoubtedly be carried on with a smaller ratio of embedded and projecting sheeting lengths. The results having a D-value of 1.18 H cannot be considered as an accurate picture of actual designs used. In many soil types such a length of embedment would make the sheeting entirely stable without top anchorage.

The fact that these tests were instrumental in discovering the economy resulting from a sand-dike backing of sheet piles must not be slighted. It is an important contribution to structural design, and the author should be given proper credit for this advance in engineering knowledge.

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DISCUSSIONS

PANAMA CANAL—THE SEA-LEVEL PROJECT A SYMPOSIUM

Discussion

BY HANS KRAMER, AND PHILIP G. NICHOLS

HANS KRAMER,[60] M. ASCE.—When the late Theodore Roosevelt, then President of the United States, received the Board of Consulting Engineers for the Panama Canal at Oyster Bay, N. Y., on September 11, 1905, he stated the main considerations of that distinguished body as follows:[2]

> "What I am about to say must be considered in the light of suggestion merely, not as direction. I have named you because in my judgment you are especially fit to serve as advisers in planning the greatest engineering work the world has yet seen; and I expect you to advise me, not what you think I want to hear but what you think I ought to hear.
>
> "There are two or three considerations which I trust you will steadily keep before your minds in coming to a conclusion as to the proper type of canal. I hope that ultimately it will prove possible to build a sea-level canal. Such a canal would undoubtedly be best in the end, if feasible, and I feel that one of the chief advantages of the Panama route is that ultimately a sea-level canal will be a possibility. But while paying due heed to the ideal perfectibility of the scheme from an engineer's standpoint, remember the need of having a plan which shall provide for the immediate building of a canal on the safest terms and in the shortest possible time.
>
> "If to build a sea-level canal will but slightly increase the risk, and will take but a little longer than a multilock higher-level canal, then of course it is preferable. But if to adopt the plan of a sea-level canal means to incur great hazard and to insure indefinite delay, then it is not preferable. If the advantages and disadvantages are closely balanced, I expect you to say so. I desire also to know whether, if you recommend a high-level multilock canal, it will be possible after it is completed to turn it into or to substitute for it, in time, a sea-level canal without interrupting the traffic upon it. Two of the prime considerations to be kept steadily in mind are—
>
> "(1) The utmost practicable speed of construction;
> "(2) Practical certainty that the plan proposed will be feasible—that it can be carried out with the minimum risk.

NOTE.—This Symposium was published in April, 1948, *Proceedings*.

[60] Brig.-Gen., U. S. Army (*Retired*), Cons. Engr., San Francisco, Calif.

[2] "Report of the Board of Consulting Engineers for the Panama Canal, 1906," U. S. Govt. Printing Office, Washington, D. C., 1906.

"The quantity of work and the amount of work should be minimized so far as is possible.

"There may be good reason why the delay incident to the adoption of a plan for an ideal canal should be incurred; but if there is not, then I hope to see the canal constructed on a system which will bring to the nearest possible date in the future the time when it is practicable to take the first ship across the Isthmus; that is, which will in the shortest time possible secure a Panama waterway between the oceans of such a character as to guarantee permanent and ample communication for the greatest ships of our Navy and for the largest steamers on either the Atlantic or the Pacific. The delay in transit of the vessels owing to additional locks would be of small consequence when compared with shortening the time for the construction of the canal or diminishing the risks in the construction.

"In short, I desire your best judgment on all the various questions to be considered in choosing among the various plans for a comparatively high-level multilock canal, for a lower-level canal with fewer locks, and for a sea-level canal. Finally, I urge upon you the necessity of as great expedition in coming to a decision as is compatible with thoroughness in considering the conditions."

Substantially that same precept, which now sounds almost prophetic, has guided the present engineering staff and consultants in the comprehensive review of the Isthmian canal situation directed by Congress on December 28, 1945. Now, as then, this waterway between the oceans must assure permanent and ample communication for the greatest ships of the United States Navy and it must accommodate all commercial traffic. The present problem consists of evaluating the military security, or invulnerability, and the traffic capacity of the Panama Canal in the direct light of past experience, both peace and war, and in the dim emanations from a crystal ball. The studies presented in the Symposium cover all aspects of the problem—comprehensively, competently, and objectively.

The writer subscribes unqualifiedly to the conclusions stated by Colonel Stratton. The optimum and the only proper course of action is the conversion of the present canal to a sea-level waterway at a cost averaging about $250,000,000 for 10 years. If it were possible to disregard national security, an expenditure of only $130,000,000 would suffice to meet the needs of commerce for the remainder of the twentieth century; but there is no sound middle ground between these two alternatives.

It is not the writer's purpose to discuss the basic considerations which entered into the engineering studies. Colonel Stratton has stated them so adequately—indeed, so eloquently—that they do not need reiteration. It is desirable, however, to mention briefly a few points of particular interest to the civil engineering fraternity.

One of the most debatable as well as intriguing aspects of the sea-level proposal, but fortunately not one of vital significance in the over-all plan, is the question of the regulation of tidal currents. It is fully recognized that tidal-control structures may be made inoperable by enemy action and therefore, willy-nilly, the canal must be dependable for open channel navigation in time of war. The writer is convinced by present evidence that tidal regulation can be dispensed with—not only in time of war but also for normal peacetime operation. However, with the problem of navigation in tidal currents still

under study, he certainly does not question the prudence of making adequate allowance, in preliminary estimates, for the eventuality of tidal-control structures—especially since they represent only about 7% of the total costs.

Undoubtedly the most challenging aspects of the sea-level conversion project, from both the engineer's and layman's viewpoints, are the stupendous excavation job and the accompanying hazard of serious earthslides. The present canal involved a maximum height of cut, vertically, of 514 ft and the removal of something less than 300,000,000 cu yd of material of which about 30,000,000 cu yd was taken out during the French regime. The conversion plan will involve cuts as high as 660 ft, vertically, and will require the removal of something more than 1,000,000,000 cu yd of material, or about four times the original undertaking. Staggering as those simple statistics may seem, modern methods and machinery have been expertly analyzed, during the course of Colonel Stratton's studies, which lead to the firm conclusion that the excavation task can be accomplished efficiently, economically, and without obstructing normal canal traffic. Those who are particularly interested in the excavation problem should read the last Symposium paper, dealing with methods and machinery for both dry and wet excavation.

The seventh paper in the Symposium constitutes a detailed report on excavation slopes and slide prevention. It yields testimony that Colonel Stratton, his staff, and his advisers have tackled the slide problem as competently and thoroughly as any individual or group possibly could. The history of earthslides in the digging of the original canal gives just cause for grave concern in judging the feasibility of much deeper excavation in the very same material. The Canal Zone is a topsy-turvey place where the sun not only rises in the Pacific and sinks into the Atlantic, but where hard bedrock is likely to be only a crust over plastic, slippery substrata; the characteristics of its geology and soils are sometimes diabolically treacherous, inconsistent, and unpredictable. Having given "the devil his due," however, it should be recognized that he is certainly not inscrutable, thanks to the modern technique of soil mechanics and much more complete knowledge of subsurface conditions.

Specifically, three broad conservative principles have been adopted in the general treatment of the slope problem in the critical Cucaracha material. The first of these is the use of generous factors of safety in the static analysis for the design of slopes; secondly, a liberal yardage allowance for the contingency of slides has been made in the basic estimates; and, finally, more detailed drilling and testing to supplement the preliminary data now available are to be performed during the construction stage to determine actual design slopes, station by station—or even closer if indicated. Despite all these precautions it would be foolhardy to assert that there will be no slides in the new excavation; but it is safe to assume that the hazard of slides will be minimized and that the engineers are justified in being confident of a successful job.

Recent political occurrences in Latin America have dramatically highlighted the Isthmian canal situation. Renewed concern for the security of the Panama Canal has brought forth proposals for new canal construction in alternative locations, on the premise that "it is unwise to carry all your eggs in one basket." That adage, however, is fallacious when applied to defensive strategy

in respect to the Panama Canal because, as was pointed out in an earlier analysis,[4] guaranteed interoceanic communication is not a matter of carrying eggs but of protecting baskets. The Panama sea-level recommendation of the Governor of the Panama Canal, as presented in Colonel Stratton's paper, has been critically reviewed at the highest levels of United States strategic policy with the irrefutable conclusion that the Panama Canal converted to sea level would be our stoutest and safest "basket."

PHILIP G. NICHOLS.[61]—The authors of the second Symposium paper have produced a clear and comprehensive dissertation on the subject matter. The conclusions reached regarding future tonnage of ocean-going commercial traffic and the conversion of this tonnage into daily peak and average transits produce extremely liberal criteria on which to establish future canal designs. Canal improvement based on these data should provide more than adequate capacity for any foreseeable contingencies, including the eventuation, in any conceivable manner, of either or both of the negative assumptions, *a* and *b*, of Roland L. Kramer (under the heading, "Ocean-Going Commercial Traffic: Future Tonnage of Ocean-Going Commercial Traffic").

The effect of the past two world wars was to decrease, markedly, the commercial tolls-paying traffic and to increase the military traffic. The amount of this increase in military traffic is unpredictable and, as the authors state, there is no assurance that in a future war the loss of commercial transits would offset completely the gain in military traffic. However, previous war experience shows that this change in preponderance from commercial to military traffic results in little variation in total transits. During the war years (1942–1946) the annual average was 6,515, whereas the average for the preceding twenty years of peace, was 6,431 transits. The traffic for the first six months of the present fiscal year indicates an annual rate of about 6,000 transits for 1948.

The great danger, from the aspect of canal capacity, has been that this increased military traffic would result in excessive peak days caused by the transit of large task forces. Advances in the sciences (such as those in the field of atomic energy), as they affect air power and new weapons, should reduce the probability of these peak days occurring. Such developments should discourage the heavy concentration of military shipping, at least in confined waters, and tend to force its dispersal. The effect of scientific developments on transportation in general is unpredictable, but the writer's opinion is that it will be to reduce rather than to increase martime freight and passenger traffic.

During the first six months of the fiscal year, 1947–1948, variations in the daily traffic volume continued to be extreme, as they were prior to World War II. The directional inequality of traffic reversed itself and is now preponderantly northbound rather than southbound. This shift may not be significant; undoubtedly, with a return to a normal, peacetime world economy, it will level off or reassume its prewar tendency. This change in directional volume is of importance to the Marine Division of the Panama Canal, since the present canal

[4] "The Isthmian Canal Situation," by Hans Kramer, *Transactions*, ASCE, Vol. 94, 1930, p. 406.

[61] Capt., U. S. Navy; Marine Supt., Dept. of Operation and Maintenance, The Panama Canal, Balboa Heights, Canal Zone.

lends itself to more efficient operation with preponderant northbound transits (rather than with the opposite), because of the existence of a commodious fog-free anchorage in the vicinity of Gatun Locks in the north, and the presence of Gaillard Cut, with its high incidence of night fog, in close proximity to the Pacific terminal. These conditions make it possible to dispatch the heavier northbound traffic through the cut during such greater part of the early daylight period as is necessary. This traffic can be anchored as required in Gatun Lake, and locked down throughout the night. By proper scheduling, the lighter south traffic can be dispatched through the cut during the shorter period remaining, and prior to the hours of high incidence of night fog. Hence, the canal has a somewhat greater one-way capacity northbound and, with two-way traffic, its capacity increases with a northerly preponderance.

The estimate of 1.5 vessels per lockage in the present canal is the least conservative in this paper. On January 13, 1939, a total of 77 warships transited north during a period of 24 hours. This force consisted of 2 large carriers, an aircraft tender, 17 cruisers, 46 destroyers, and 11 submarines. All southbound traffic was discontinued until the next day. These transits were accomplished with 49 lockages, or an average of 1.57 ships per lockage. This later value tends to make the estimate of 1.5 ships per lockage seem safe, but conditions were ideal as to number of ships in each length class. Delays to individual units were acceptable if they expedited the transit of the entire force. On January 10, 1939, a peak day, a total of 35 ships transited the canal; 22 transits were northbound, with 18 lockages or 1.22 ships per lockage, and 12 transits were southbound, with 11 lockages or 1.09 ships per lockage. By accepting delays up to several hours the transits could have been effected with more tandem lockages, increasing the ships per lockage somewhat, but not approaching the estimate of 1.5 unless delays into the next day were accepted.

Messrs. Johnson and Steinborn state that the present canal capacity can be increased to 70 vessels per day by minor improvements, such as the installation of tie-up stations in Gaillard Cut; the modification of lock gates, gate settings, and culverts; and the provision of modern electronic aids to navigation in the cut. This conclusion is based on a number of assumptions with which the writer is in full agreement, but the use of the unit 1.5 ships per lockage for planning purposes appears overly optimistic unless substantial delays are imposed. One might argue that the delays will have to be accepted. If this is granted the situation may tend to rectify itself, because the imposition of delays may result in a loss of traffic. Upon release of the biennial lock overhaul schedule for the winter of 1947–1948, several local steamship agents inquired as to the probability of delays to shipping. They stated that their principals, engaged in shipping between the Antipodes and Europe, had intimated that delays would result in rerouting via the Straits of Magellan or the Suez Canal. This trend of thought was probably considerably influenced by the dollar scarcity existing in all European maritime nations. The presence of such thinking is indicative of the necessity to increase capacity if the canal is to continue to provide adequate service to the shipping industry.

A completely modernized canal with an increase in lock sizes to 200 ft by 1,500 ft, the widening of the cut, and the provision of a Pacific summit-level

anchorage would greatly increase the operational efficiency and capacity of the canal. This capacity is sufficiently in excess of the predicted traffic so that the lack of conservation in basic assumptions, if there is any, is more than compensated for. Further discussion of this plan is perhaps beyond the scope of the Symposium. However, the magnitude of the task in man power and material makes it prohibitive if the gain is only in capacity (ship dimensions and traffic volume) and operational facility. Only two commercial vessels, the *Queen Elizabeth* and the *Queen Mary*, cannot be accommodated by the present canal. There is no evidence of plans to duplicate these mammoth liners; in fact, from contacts in the maritime industry, the writer infers that the trend is away from large express passenger liners approaching their dimensions. The Cunard White Star liner *Caronia*, launched in 1947, has an over-all length of 715 ft and a beam of 91 ft. Her gross tonnage is less than that of the new *Mauretania* because the technical developments of recent years have enabled ship designers to produce more earning capacity per ton of ship than was possible in the past. This trend (away from large ships of the Queen class) will receive further impetus from the expansion of overseas air services.

If this reasoning is sound the necessity for increasing the dimensional capacity of the canal must be predicated on combat ship sizes; the problem then becomes one of national defense and a prime requisite is security. Little or no increase in security can be obtained by any high-level lock canal.

The capacities of a Panama sea-level canal are arrived at in the paper by what appear to be conservative assumptions. As in the completely modernized lock canal, the capacities obtained are so far in excess of the predicted volume of traffic or ship dimensions that any errors in the assumptions are covered adequately. The operational difficulties experienced in the present canal, with the exception of fog hazards, are removed. The fog area will undoubtedly be increased, but there is no evidence that prevalence of fog from a daily durational or seasonal aspect will suffer any appreciable change. The operational difficulties imposed by the introduction of tidal current and the extension of the fog area are more than compensated for by the improved width, depth, and alinement of the channel and by the provision of tidal regulation with tie-up stations in the channel above the lock and navigable pass. The continued improvement in ship design, as it produces better maneuverability, and the further development of aids to navigation (particularly in the electronic field) will tend to reduce the hazards of ship cperations, in general, and those hazards peculiar to all narrow waterways, in particular. Operation without tidal regulation is entirely feasible, and experience will indicate that this method can be adopted with safety, except in unusual circumstances.

The writer concurs in the final summary of the Johnson-Steinborn paper, as far as it relates to the capacities obtained by converting the present canal to a sea-level canal, or by completely modernizing it to a terminal lake canal. He doubts the wisdom of depending on the minor modifications to provide adequate canal capacity through the remainder of the twentieth century, if Professor Kramer's predictions of traffic growth are accepted. The minor improvements described would not increase the security or the dimensional capacity but should provide sufficient canal capacity to serve commercial

shipping for many years—if not through the entire century. Canal improvement in excess of such minor modifications should be based entirely on the predicted effect of present and future scientific developments, on commercial and combat ship dimensions, and on canal security. The question of security seems relatively simple to answer. The only method of reducing the over-all vulnerability of the canal to present or projected weapons is a change to a sea-level canal. The extent of this gain in security is important; perhaps it is so small in comparison to the cost that the possibility of damage should be accepted as a calculated risk. The effect from a dimensional aspect is more involved and less easy to predict. Logical reasoning or intuition (whichever one prefers to call it) induces the writer to believe that the effect of scientific progress will tend to make numbers of ships, both commercial and combatant, more profitable and more valuable than the great size of individual units. The effect of the new sciences on weapons and on shipping, both commercial and military, should be the subject of exhaustive study, because this is the crux of the problem.

Corrections for *Transactions:* In April, 1948, *Proceedings*, on page 446, insert Fig. 3 from page 447 instead of the present Fig. 2 and correct its caption to read "Fig. 2.—Panama Canal Transits"; on page 446, line 26, change "daily traffic" to read "canal transits"; on page 447, insert Fig. 2 from page 446 instead of the present Fig. 3 and correct the caption to read "Fig. 2.—Predicted Canal Traffic"; in Tables 1 and 2, Cols. 1, change route 19 to route 17; on page 457, line 22, change "beaches" to "reaches"; on page 511, line 9, change the last word from "their" to "the"; transpose pages 560 and 561; on page 576, line 3, insert a comma after "pyroclastic"; on pages 599 and 600, interchange the main captions of Figs. 82 and 83; on pages 604 and 605, leaving the captions in place, interchange the graphs of Figs. 91 and 92 but the tabular matter above the caption of Fig. 92 belongs with Fig. 92; and on page 623, line 17, change "Cuba" to "Yuba."

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DISCUSSIONS

EXPERIMENTAL DETERMINATION OF VIBRATION CHARACTERISTICS OF STRUCTURES

Discussion

BY R. K. BERNHARD, CARL A. LUDEKE, AND M. J. TURNER

R. K. BERNHARD,[4] M. ASCE.—The method described by the writer presents an ingenious procedure for determining certain vibration characteristics of actual structures. The outstanding simplicity of application is one of its most important advantages.

The question the writer would like to raise is: Which of the many significant and complex dynamic phenomena, occurring in numerous practical cases, can be determined by the new method? Any convenient nondestructive procedure which permits the determination of static and dynamic characteristics by observations on the structure itself is preferable, in many cases, to methods involving either undersized or oversized models. The change in size from the actual structure to the model very often includes, besides other disadvantages, an undesirable compromise with respect to the various similarity scales.[5]

However, any method involving the use of the prototype structure must simulate actual service conditions as closely as possible. This stipulation can be explained by a few practical examples referring to the magnitude of excited force amplitudes, excited displacement amplitudes, and the required energy input to produce these amplitudes.

Nonharmonic Vibrations.—If the writer understands this paper correctly, the method induces extremely small sinusoidal vibrations in the structure under investigation. Exciter mass and excited mass are coupled loosely by the elasticity of the pendulum springs.

Numerous investigations by means of induced sinusoidal vibrations,[6] however, with a rigid coupling between exciter and excited mass, have shown a logarithmic damping decrement (θ) in bridges, for example, of the order:

NOTE.—This paper by Samuel J. Loring was published in December, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1948, by Alexander Klemin.

[4] Prof. of Eng. Mechanics, Rutgers Univ., New Brunswick, N. J.

[5] "Introduction to Engineering Analysis," by J. J. Slade, Edward Brothers, Inc., Ann Arbor, Mich., 1947.

[6] "Determination of the Static and Dynamic Constants by Means of Response Curves," by R. K. Bernhard, *Journal of Applied Physics*, December, 1941, pp. 866–874.

$\theta = 0.1$. For an exciter mass (pendulum) of, say ,1 lb, the amplification factor at resonance ($A = \frac{\pi}{\theta} = 30$) would yield an effective dynamic force of 30 lb. For simplicity, consider the exciter and excited masses as a two-mass system without damping, and assume a medium size structure weighing 200 tons. In the case of a loosely coupled two-mass system a deflection amplitude of $\frac{1}{2}$ in. of the exciter mass (pendulum) may build up maximum amplitudes in the excited mass (structure) of $\frac{1}{2} \times \frac{30}{400{,}000}$, which is approximately $\frac{1}{27{,}000}$ in. To the writer, this value appears smaller than the deflection amplitudes to be expected in many cases under field service conditions.

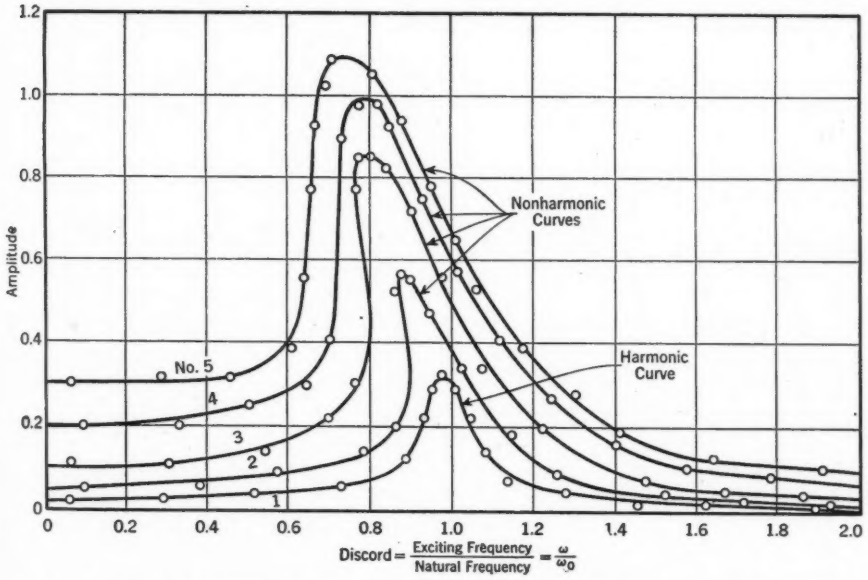

FIG. 3.—RESONANCE CURVES FOR NONHARMONIC VIBRATIONS EXCITED BY INDUCED VIBRATIONS (AMPLITUDES VERSUS DISCORD FOR VARYING MAGNITUDES OF THE EXCITING FORCES)

Larger deflection amplitudes, however, may produce nonharmonic vibrations, which do not represent exceptional cases, as is often assumed. The five resonance curves, shown in Fig. 3, are taken from experiments on actual structures, excited by means of induced sinusoidal vibrations with increasing displacement amplitudes, which seem to prove this statement.[7] For a simple girder a series of resonance curves has been determined, showing the relation between the discord (that is, the ratio of exciter frequency to natural frequency of the girder) and the corresponding excited deflection amplitude of the girder. The magnitude of the exciter forces has been varied.

Curve No. 1 is plotted for a small exciter force in order to produce a maximum deflection amplitude within the linear range—that is, for constant spring elasticity and constant damping. Hence, this particular curve still has a

[7] *Journal of Applied Mechanics*, A.S.M.E., June, 1939, p. 91, Fig. 1.

normal form. Only with larger deflection amplitudes will nonlinear elasticity and nonlinear damping appear, mainly because of minute slipping action in the connections or the supports or both. Thus, the next curve (No. 2) shows a slight tilting effect. This effect is increased in curve No. 3, which is produced by a substantially larger exciter force. A wider gap at the top of curve No. 3 (still more pronounced in curve No. 4) indicates a considerable increase in damping. The tilting of the three curves (Nos. 2, 3, and 4) toward lower frequencies is characteristic of nonharmonic vibrations.

Finally, in curve No. 5, this tilting effect gradually disappears because of larger damping. However, the broadening of this curve in the resonance range, combined with the steep incline in the rising branch, produces also a nonsymmetrical shape. With increasing frequencies, an abrupt step up from small to high amplitudes takes place. This step up indicates a state of unstable equilibrium. With decreasing frequencies, a similar abrupt step down from high to low amplitudes occurs, although it takes place at a point of larger forces as compared with the step up. Hence, for curves No. 2 and No. 3, at discords around 0.9 and 0.75, respectively, three different ranges of unstable dynamic equilibrium exist.

In this connection it might be mentioned that the Tacoma Bridge failure[8,9,10,11,12,13,14,15,16] was caused also by a state of unstable equilibrium, in that case, due to aerodynamic instability. These characteristic dynamic phenomena will not appear under small deflection amplitudes, producing curves of type No. 1 only.

Coupling and Beat Effects.—The active participation in the vibration of supports, adjacent structures, and surrounding soil may influence the dynamic characteristic substantially. Fig. 4 shows frequency versus energy-input curves for a railroad bridge, consisting of several spans of 230 ft each.[17] The curves on the right side of Fig. 4 represent resonance curves for one of the excited spans. These curves indicate a pronounced coupling effect (two-wave resonance curves) between the excited span and the adjacent span, loosely coupled by way of the intermediate pillar and foundation, as follows:

Description	Unloaded condition	60-ton locomotive
Natural frequency, in cycles per second	3.47	2.97
Logarithmic damping decrement	0.063	0.063
Energy input, in watts	460	160
Spring constant, in tons per inch	216	....
Reduced mass, in tons	179	....
Coupling coefficient between adjacent spans	0.015	....
Vibrating mass of foundation, plus adjacent soil, in tons	12,600	....

[8] "Rigidity and Aerodynamic Stability of Suspension Bridges," by D. B. Steinman, *Transactions*, ASCE, Vol. 110, 1945, p. 439.
[9] *Ibid.*, p. 476.
[10] *Ibid.*, p. 485.
[11] *Ibid.*, p. 487.
[12] *Ibid.*, p. 490.
[13] *Ibid.*, p. 522.
[14] *Ibid.*, p. 532.
[15] "Failure of the Tacoma Narrows Bridge: Report of the Special Committee of the Board of Direction," *Proceedings*, ASCE, December, 1943, p. 1555.
[16] Discussion by Louis Balog of "Failure of the Tacoma Narrows Bridge: Report of the Special Committee of the Board of Direction," *ibid.*, November, 1944, p. 1475.
[17] "Purely Dynamic Methods for Determination of Stresses in Structures," by R. K. Bernhard and W. Späth, *Der Stahlbau*, March, 1929, pp. 61–68.

A simple calculation yields that the dynamically effective mass between the adjacent spans amounted to approximately 12,600 tons. This dynamically effective mass consisted of the mass of the pillar, the concrete foundation, and the soil surrounding this foundation, all three masses participating in the vibrations. The vibrating soil mass was approximately ten times larger than the enclosed concrete foundation.

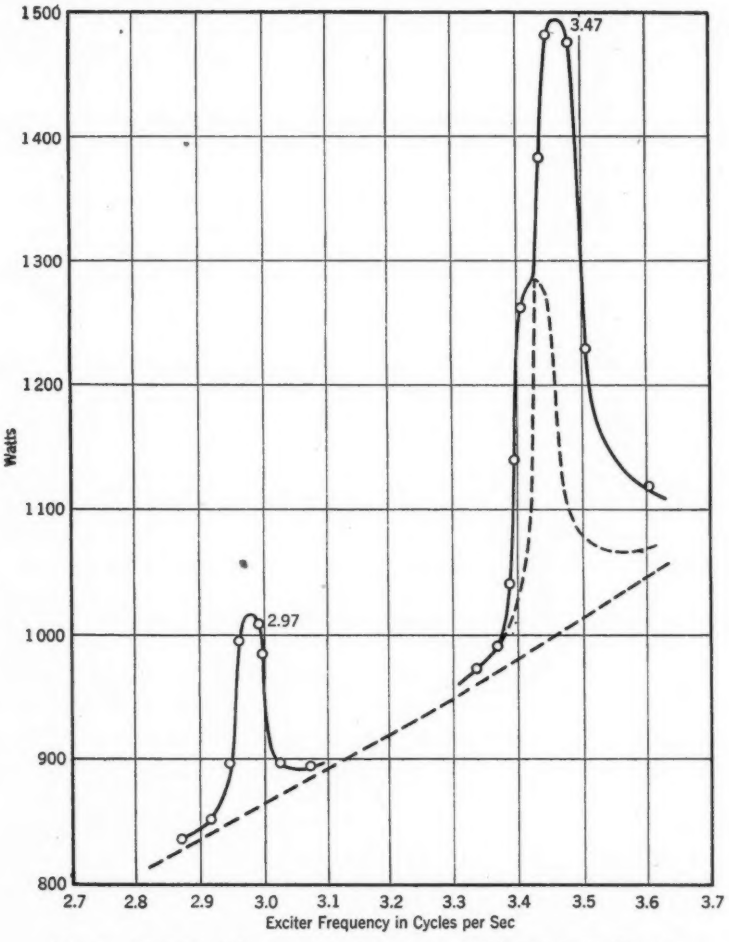

FIG. 4.—FREQUENCY POWER INPUT CURVES OF 230-FT RAILROAD BRIDGE

Detuning the excited span by loading it with a light locomotive (60 tons) in the center yielded undistorted resonance curves of the excited span as shown on the left side of Fig. 4. The reduced natural frequency of the loaded span prevented effectively any observable coupling effects from the adjacent span. Stress-time diagrams of observations in the upper chords of highway trusses[18,19] during the passage of trucks have revealed similar coupling phenom-

[18] "Dynamic Relations Between Moving Loads and Structures," by R. K. Bernhard, *Mechanical Engineering*, September, 1938, pp. 696–702.

[19] *Ibid.*, p. 701, Fig. 11.

ena. Pronounced "beat" effects in such diagrams indicate the characteristic transfer of energy from the exciter mass (vehicle) to the excited mass (truss), and, conversely, this holds true. In all cases, particularly where dynamically effective masses in the order of 10,000 tons and more are participating in the vibrations and must be excited, a substantial amount of energy is required to produce these phenomena.

In the writer's opinion these few examples demonstrate that, in many cases, the determination of the dynamic characteristics by tests on actual structures yields rather complex phenomena. These can be neither obtained nor interpreted correctly, unless actual service conditions are reproduced faithfully. This is true particularly as far as the magnitude of the excited deflection amplitudes or as far as the energy input, required to excite the desired vibrations, are concerned.

With respect to the application of the author's method, the writer would like to ask the following questions:

1. Keeping in mind the necessity of simulating actual service conditions, how heavy a structure can be investigated by increasing the size of the pendulum?
2. Can nonharmonic phenomena (which are often predominant in actual cases), including dynamically unstable conditions, be evaluated?
3. What methods have been used to record the decay curves of the vibrating pendulum? and
4. If an air gap of an electrostatic or an electromagnetic type of pickup unit forms the only coupling link between pickup and pendulum, can the restoring force of the recording mechanism be reduced sufficiently? In other words, does not the restoring force of the pickup unit distort the decay diagram (or "beat-diagram") of the pendulum? Photographing the pendulum indicator with a high speed motion picture camera might avoid these difficulties.

CARL A. LUDEKE,[20] ESQ.—By an interesting application of D. Bernoulli's principle of the superposition of vibrations Mr. Loring reduces the testing of the vibration characteristics of structures to a set of problems in two degrees of freedom. Each of these problems involves the manner in which the rectilinear motion of the damped vibrations of a small test mass is affected by the vibration characteristics of the structure in the direction of motion of the test mass and at the point of testing. A splendid application of the theory of elastic vibrations for systems with small damping shows that the motion of the test mass can be considered as a vibration of frequency ω_o, which has a varying amplitude A. It is obvious that A must account for the damping in the analyzer, the damping in the structure, and the "wandering" of energy between the test mass and the structure, which is to be expected in a system having two degrees of freedom. Results of the analysis show that A can be considered in the form,

$$A = A_o e^{-\frac{1}{2}\omega_o \sigma_o t} \psi \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (54)$$

If the structure is rigid or if the natural frequency of the test mass is more than slightly different from the natural frequency of the structure at the point

[20] Associate Prof. of Mechanics, Univ. of Cincinnati, Cincinnati, Ohio.

of test and in the direction of the test mass motion, then Eq. 54 is of such a form that ψ must equal one. However, ψ has very interesting characteristics when the structure is not rigid and these two natural frequencies are "tuned"—that is, they are approximately equal. The function ψ is defined by Eq. 30, and, depending on the value of the damping index, G, it assumes three distinct forms when exact tuning is obtained. These forms are shown in Eqs. 34, 36, and 38. Since the characteristics of ψ determine the equations which are used when operating the analyzer, it seems of interest to point out that ψ itself can be considered as a free damped vibration; that is, it satisfies a differential equation of the form,

$$a\,\psi'' + b\,\psi' + c\,\psi = 0 \qquad (55)$$

The three forms of ψ correspond to the solutions of Eq. 55 for the well-known cases in which damping is less than, greater than, or equal to critical damping.

To show this, it should be noted that, when the damping is less than critical, the solution of Eq. 55 satisfying the initial conditions, when $t = 0$, $\psi = 1$, and $\psi' = 0$, can be written in the form,

$$\psi = e^{-\frac{1}{2}K\,t}\left(\cos n'\,t + \frac{K}{2\,n'}\sin n'\,t\right) \qquad (56)$$

in which

$$K = \frac{b}{a} \qquad (57)$$

$$n' = \sqrt{n^2 - \tfrac{1}{4}K^2} \qquad (58)$$

and

$$n^2 = \frac{c}{a} \qquad (59)$$

Eq. 34 is in this form, with the values of a, b, and c such that Eq. 55 becomes

$$m_o\,\psi'' + \sqrt{\frac{m_o\,k_o}{r}}\,G\,\psi' + \frac{k_o}{4\,r}\,\psi = 0 \qquad (60)$$

When the damping is greater than critical damping, Eq. 55 has a solution,

$$\psi = C_1\,e^{s_1 t} + C_2\,e^{s_2 t} \qquad (61)$$

in which

$$s_{1,2} = -\frac{b}{2\,a} \pm \sqrt{\frac{b^2}{4\,a^2} - \frac{c}{a}} \qquad (62)$$

If Eq. 55 is again taken in the form of Eq. 60, and if the same initial conditions are satisfied, then C_1, C_2, s_1, and s_2 take the forms required to make Eq. 61 the same as Eq. 36.

If the damping equals the critical damping, the solution of Eq. 55 assumes the form,

$$\psi = (C_1 + C_2\,t)\,e^{-b\,t/2a} \qquad (63)$$

If Eq. 55 is in the form of Eq. 60 with G equal to 1, and if the same initial conditions are satisfied, then Eq. 63 becomes

$$\psi = \left(1 + \frac{1}{2}\frac{\omega_o}{\sqrt{r}}\,t\right)e^{-\omega_o t/2\sqrt{r}} \qquad (64)$$

which for large values of t assumes the form of Eq. 38.

Thus, the amplitude of motion of the test mass is given by Eq. 54, in which, at exact tuning, the function ψ must satisfy the conditions when $t = 0$, $\psi = 1$, and $\psi' = 0$, and must be a solution of the differential equation

$$m_o \psi'' + \left(\frac{\beta - \beta_o}{2} \right) \psi' + \frac{k_o}{4r} \psi = 0 \quad \ldots\ldots\ldots\ldots\ldots (65)$$

in which β is the coefficient of viscous damping of the structure in the mode under observation, and β_o is the coefficient of viscous damping of the test mass.

M. J. TURNER,[21] ESQ.—During 1947 an instrument of the type described by the author has been used to determine vibration characteristics of the tail surfaces on a small airplane. Although experience is rather limited, the analyzer appears to have excellent potentialities for the accurate determination of natural frequencies and structural damping. The simplicity of the instrument and its relatively small size are particularly attractive when vibration surveys of limited scope have to be made outside the laboratory. For large-scale laboratory testing, in which vibration measurements at many points on the structure are required, the more elaborate electronic instrumentation in current use is definitely preferable. Even in this sort of work the analyzer is very useful for supplementary checking.

In the vibration testing of airplane structures, the frequent occurrence of several vibration modes with closely spaced natural frequencies is a source of great difficulty, and it is felt that the potentialities of the analyzer for resolving such difficulties should be systematically explored. Possibly the scheme presented by Mr. Loring for interpretation of test mass motions is adequate in almost all practical situations, but further investigation will be required to confirm that point. In view of the analytical complications which occur when only one mode of the structure is considered, it appears that a theoretical study of test mass motion under the simultaneous influence of two or more structural modes would be very difficult. However, it would be entirely feasible to determine experimentally the potentialities of the instrument for measurement of closely spaced natural frequencies. Probably it would be desirable to build a special structure for the purpose, with provision for adjustment of two of its natural frequencies to any desired degree of proximity. Then, by locating the analyzer at a nodal point of one of the interfering modes, it would be possible to determine the natural frequency of the other mode precisely. Further tests with the analyzer mounted at other points, where both the modes can be excited, would serve to determine the limits of its resolving power.

Finally, it should be noted that the test mass motion may be influenced by the excitation of vibration modes of the analyzer itself. The frequencies of these modes are indicated by very sharp dips in the plot of decay time, $(t_\theta)_o$, against frequency, obtained with the analyzer mounted on a rigid base. The existence of these extraneous modes has not proved to be a serious handicap in the use of the instrument.

[21] Supervisor of Dynamic Analysis, Chance Vought Aircraft Div., Stratford, Conn.

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DISCUSSIONS

ESTIMATING DATA FOR RESERVOIR GATES

Discussion

BY FRANK L. BOISSONNAULT

FRANK L. BOISSONNAULT,[21] ESQ.—The fine reception accorded this paper is very gratifying to the writer and the many commendatory remarks in regard to it, both in the published discussions and in personal correspondence, are greatly appreciated.

Possibly it would be in order to further clarify the method of approach used in the paper, which will serve to answer some of the points brought up in the discussion. The approach is empirical; the data, on as many different gates of the given type and covering as wide a range as possible, were carefully checked to be sure that the same parts were included and that all, therefore, were properly comparable. These data were then correlated graphically by the methods described so that conclusions could be drawn for any set of conditions within the range of the data. The expressions used as abscissas do not purport to be rational formulas for the weight of a gate, but are empirical expressions developed for the purpose of correlation. Therefore, the results are the averages of those which actual practice has shown to exist under present and past states of the art, rather than any individual's idea of what they should be theoretically. Consequently, such details as working stresses, length of radii in Tainter gates, etc., are beyond the scope of these charts, which were developed for making quick preliminary estimates.

As brought out in the "Introduction," all the information is given on each point and its position plotted, so that the engineer can use his own judgment in intelligently comparing past practice with the case in hand.

It is believed that too much refinement as to design details is neither warranted nor even desirable because most engineers would rather be a little on the safe side in preliminary estimates. Reliable data of this character, which have sufficient detail and a wide enough range of sizes to establish curves, are very difficult to obtain. The writer has scoured every public source of

NOTE.—This paper by Frank L. Boissonnault appeared in September, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1948, by Theodore B. Rights; March, 1948, by George R. Latham, and D. A. Buzzell; and May, 1948, by A. E. Niederhoff, and Joseph R. Bowman.

[21] Engr., U. S. Engr. Office, Los Angeles, Calif.

such information which suggested itself and has used all that could be found in the preparation of the paper. Such data on many other types of gates simply do not exist in the ordinary sources. Neither time nor available data permit the introduction of additional material to the paper at this time.

All the discussions as well as private correspondence addressed to the writer reveal a demand for estimating data on vertical-lift gates in similar form to those presented in the paper. Mr. Bowman's discussion presents some weight and cost data. These, together with Mr. Niederhoff's corrected cost formula and his previous paper on this subject,[15] should be of value in estimating gates of this type.

Mr. Neiderhoff takes exception to the use of the charts as "checks to compare final designs with those compiled previously," as suggested in the "Summary" of the paper. In this statement, it was taken for granted that the construction methods and design factors of the new gate would be similar to those used in the chart gates or that the engineer would use his judgment in making suitable allowances for deviations in type of construction. Possibly an example to illustrate the intent of this statement might be in order. Reference is made to the point for the Sepulveda Dam slide (Southern California) gate in Fig. 2(*b*). This is a 6-ft by 9-ft gate with a maximum possible head of only 55 ft, yet its weight is more than 112,000 lb, which makes it the heaviest slide gate of which there is any record. There was nothing in the requirements for this gate which called for special construction. It seems reasonable to believe that, if the designer had then had this chart available to compare the design with previous practice, the plans would have been scrutinized in detail to determine the reasons for the excessive weight. These reasons could easily have been found.

The writer would also like to comment on a few other points raised by Mr. Bowman. Mr. Bowman observes that the function in Eq. 1 used as the abscissa in Fig. 2 for slide gates "appears to be rather complicated for estimating purposes." It was realized that this expression was too complex for a simple slide rule operation. Attention is called to the simple graphical solution of this function in the upper part of Fig. 2, which was developed by the writer for this very reason. It is believed that the complexity of the function is of no moment as long as the result can easily and quickly be obtained graphically, especially when it gives as good results as this one does. Of course, the scale has been reduced in the printing from the original 10½-in. by 12-in. chart so that it is quite small for much practical use. Similar comment applies to Mr. Bowman's remarks relative to the Broome-gate curves.

Mr. Buzzell's comment on a new design for Tainter gates, developed by the Unites States Engineer Corps, is interesting and the writer would appreciate specific information on the particular gates to which he refers. It is noted in this connection that the weight of the Lytel Creek Tainter gate, which was designed by the Los Angeles Office of the U. S. Engineers since the preparation of these charts, is in quite close agreement with the weight which would be predicted by the welded gate curve of Fig. 4. This gate, now under construction,

[15] "Vertical Lift Gate Selection," by A. E. Niederhoff, *Western Construction News*, February, 1944. pp. 73–75.

was designed to conform to the then latest Engineer Corps standards, and has inclined side frames as mentioned in Mr. Buzzell's remarks.

Summary.—Most of the data available to the writer have been utilized in the preparation of the paper. This type of material is very difficult to obtain and it is hoped that members of the profession will cooperate to gather information on other types of gates.

As brought out in the foregoing, data in sufficient detail and range on existing gates or finished designs are not available from ordinary sources. Nevertheless, many engineers have information available to them on an individual project with which they are, or have been, connected. The writer would be glad to collect and correlate such data sent to his personal attention. Such an arrangement would soon result in building up a volume of data on a range of gate sizes and types, even though any one contribution in itself would not have much significance.

In closing, the writer wishes to thank the gentlemen participating in the discussion for their interest and their many valuable suggestions.

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DISCUSSIONS

INVESTIGATION OF DRAINAGE RATES AFFECTING STABILITY OF EARTH DAMS

Discussion

BY W. L. SHANNON, AND F. H. KELLOGG

W. L. SHANNON,[25] ASSOC. M. ASCE.—In connection with the design of a rolled fill earth dam in New Hampshire in 1945, the writer became interested in investigating the time required for the drainage of the pervious upstream shell section of a rolled earth fill dam following drawdown of the upstream pool. This problem is of general interest because most earth dams of modern construction contain substantial upstream pervious shell sections. To investigate the stability of such dams it is usually necessary to make some assumption regarding the drainage of this shell section for the condition of drawdown of the upstream pool.

At the writer's suggestion an approximate solution for the drainage of an upstream shell section of a dam was developed by A. Casagrande, M. ASCE, and presented in an unpublished report, dated May 23, 1945, to the Boston (Mass.) District, Corps of Engineers, Department of the Army. Professor Casagrande assumes that the upstream pool elevation is suddenly drawn down the depth D (Fig. 22). This is a reasonable approximation providing the time for a small percentage of drainage of the shell section is much greater than the actual time for full drawdown of the pool. It is further assumed that the free water surface during drainage may be approximated by inclined straight lines passing through the toe of the shell. This approximation has been found to be reasonable where the seepage quantity is of interest and the shape of the free surface is not. At the start of drainage the free water surface is at the upstream face of the shell; at any time after drainage starts the free water surface is the line designated t_U in Fig. 22.

NOTE.—This paper by F. H. Kellogg was published in September, 1947, *Proceedings*. Discussion on this paper has appeared in *Procedings*, as follows: February, 1948, by Harry R. Cedergren; and May, 1948, by Ross M. Riegel, and T. W. Lambe.

[25] Research Associate in Soil Mechanics, Graduate School of Eng., Harvard Univ., Cambridge, Mass.

The volume of water which will drain from the shell in the time dt (Fig. 22) is equal to the volume of the triangular slice times the yield, w_y:

$$dq = -\frac{dh}{2}(L + h \cot \beta)\, w_y \quad \ldots\ldots (59)$$

Based on Darcy's law and making Dupuit's assumption,[26] which is reasonable for slopes of the upstream face less than 30°, the quantity of flow per unit of time may be approximated by

$$\frac{dq}{dt} = k\, a\, i = k\left(\frac{h}{2}\right)\left(\frac{h}{L + h \cot \beta}\right) \quad \ldots\ldots (60)$$

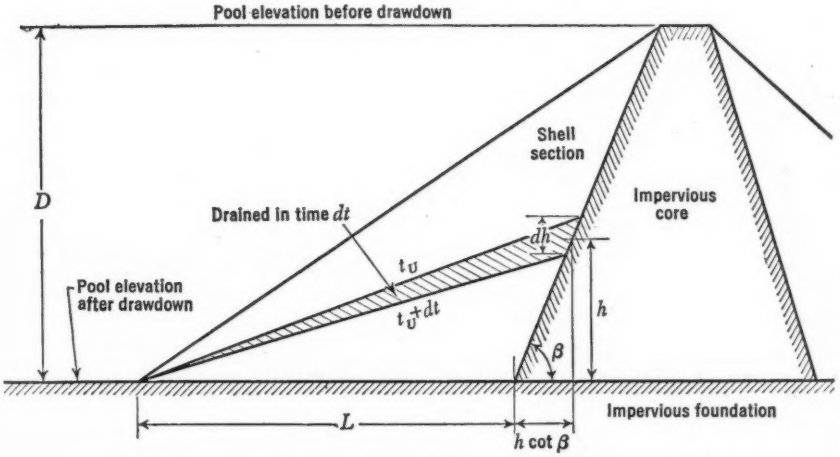

FIG. 22.—DRAINAGE OF UPSTREAM SHELL SECTION OF AN EARTH DAM

Eqs. 59 and 60 when solved and integrated may be reduced to the following form:

$$T = \frac{U}{100 - U} + J \log_e \frac{100}{100 - U} + J^2 \frac{U}{100} \quad \ldots\ldots (61)$$

in which U is the percentage of drainage, the ratio, in percentage, of the volume drained to the total volume; and J and T are dimensionless quantities defined as follows:

$$T = \frac{t_u\, k\, D}{w_y\, L^2} \quad \ldots\ldots (62)$$

and

$$J = \frac{D}{L} \cot \beta \quad \ldots\ldots (63)$$

If β equals 90°, Eq. 61 reduces to

$$T = \frac{U}{100 - U} \quad \ldots\ldots (64)$$

[26] "Seepage Through Dams," by A. Casagrande, "Contributions to Soil Mechanics 1925–1940," Boston Soc. of Civ. Engrs., Boston, Mass., 1940, p. 304.

It was suggested by Professor Casagrande that viscous fluid models could be used advantageously to check this theoretical solution. Viscous fluid models have several important advantages over sand models, such as those used by the author, for the study of nonsteady flow problems. In a viscous fluid model all the fluid is drained, whereas in sand models there is a variable quantity of water retained by capillarity. The coefficient of permeability of the viscous fluid model is the same in every direction and at every point.

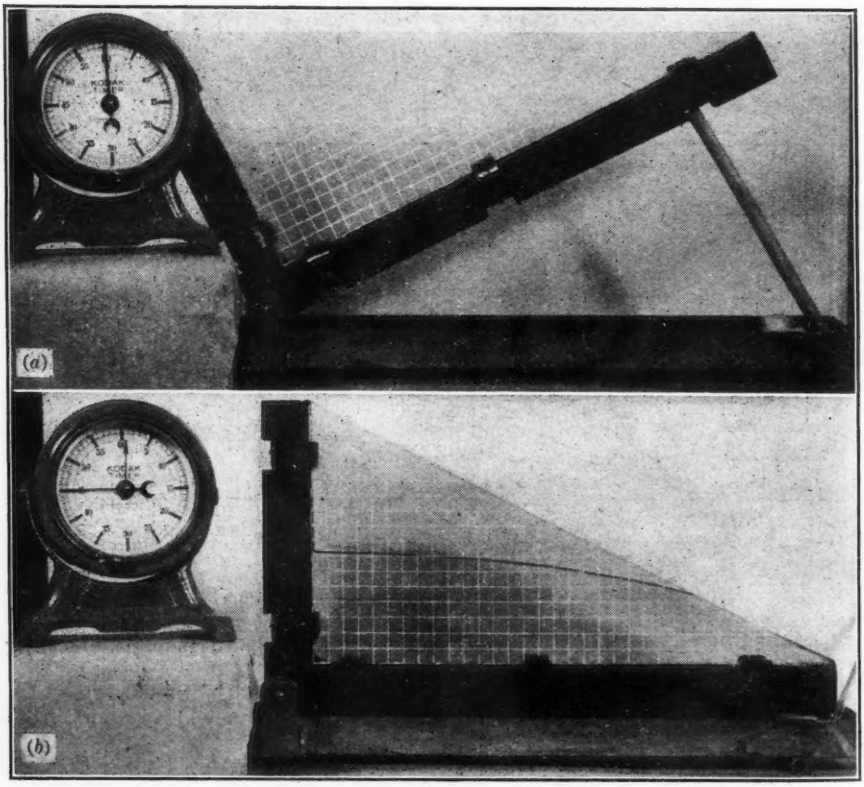

FIG. 23.—VISCOUS FLUID MODEL
(*a*) Inclined for Filling
(*b*) During Drainage Test

Different coefficients of permeability for different elements of a structure may be obtained by varying the plate spacing. In sand models it is difficult to place the sand so that it has the same coefficient of permeability throughout. To obtain two or more sands having desired ratios between their coefficients of permeability usually is extremely difficult.

A viscous fluid model was designed and constructed at Harvard University (Cambridge, Mass.) and tests were performed under the supervision of the writer at the Soil Mechanics Laboratory of the Boston District, Corps of Engineers. The model, as shown in Fig. 23, represents the upstream shell of a

dam resting on an impervious horizontal foundation and having an impervious core with a vertical upstream face. The upstream face of the shell has a slope of 1 vertical and 2 horizontal. The model consists of two triangular parallel glass plates 0.28 cm apart and held in a frame which provides the impervious horizontal and vertical boundary. To fill the model with fluid the frame was tilted so that the sloping open edge was horizontal, as shown in Fig. 23(*a*).

The fluid used was glycerine, which is sufficiently viscous to insure laminar flow between the plates. Glycerine has an affinity for glass, and hence, as drainage of the fluid occurs, a thick film will cling to the glass plates in the drained area unless precautions are taken to reduce this affinity. In these tests, the glass plates were given a very thin coating of paraffin, which reduced to a negligible amount the quantity of glycerine clinging to the plates.

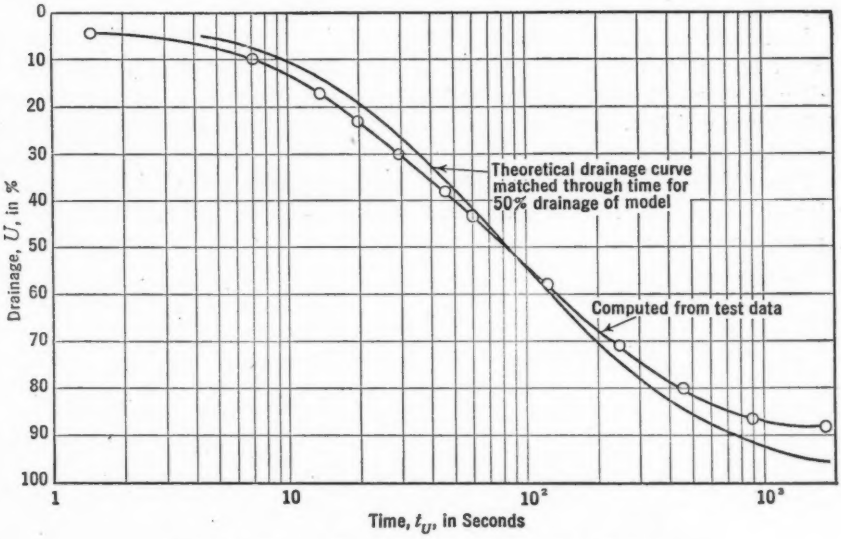

FIG. 24.—VISCOUS FLUID MODEL DRAINAGE TEST RESULTS

A drainage test consists in suddenly tilting the model filled with fluid from the position shown in Fig. 23(*a*) to that shown in Fig. 23(*b*). Periodic observations of the position of the free surface were recorded photographically. In Fig. 23(*b*) the dark line at midheight of the model is the free surface of the glycerine at 45 sec after start of drainage. The results of a drainage test on this model are shown in Fig. 24.

In order to determine a value for the coefficient of permeability of the glycerine flowing between the two paraffin coated glass plates, measurements of the fluid viscosity are necessary. For the model tests performed, measurements of fluid viscosity were not obtained; hence, a direct comparison between Eq. 64 and the model test results is not possible. However, it is possible to match a theoretical curve based on Eq. 64 through any desired point on the drainage curve for the model. In Fig. 24 a theoretical curve has been matched through the time for 50% drainage of the model. The good agreement between the test

data and the matched theoretical curve indicates that the derivation is reasonable as far as shape of the time-drainage curve is concerned.

In connection with an extensive viscous fluid model investigation of a very similar problem, a good agreement, both with respect to the shape and the position of drainage curves, was obtained between curves determined from viscous fluid model tests and drainage curves computed from theoretical equations which were derived similarly to Eq. 61. Thus, it is concluded that Eq. 61 may be used to estimate the time-percentage drainage curve of a shell section of an earth dam for the condition of sudden drawdown of the upstream pool. As neither the coefficient of permeability nor the yield are constants, it is considered reasonable to assume two limiting values for each factor and to compute the time-percentage drainage curve as a band within which the actual curve will probably fall.

From such a solution, the percentage of drainage corresponding to any desired time after sudden drawdown of the upstream pool may be estimated. This value for percentage of drainage will be reasonable only if the percentage of drainage which occurs during the actual time required for pool drawdown is small.

By making several trial flow nets, one net may be constructed for a desired value of percentage of drainage. Fig. 25 is an example of a flow net representing seepage conditions in an upstream shell of a dam which has drained to $U = 16\%$.

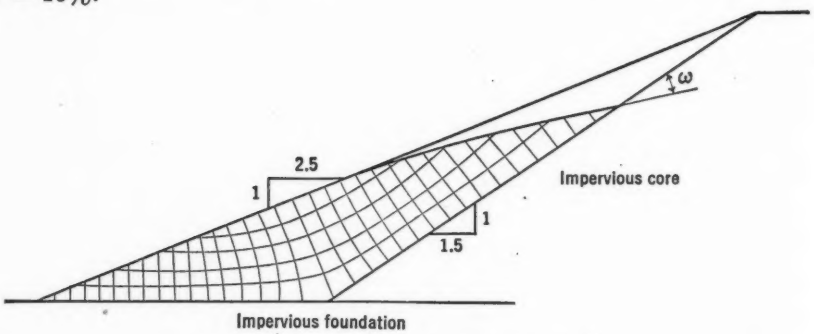

FIG. 25.—EXAMPLE OF FLOW NET AT 16% DRAINAGE

According to Professor Casagrande, the problem of constructing a flow net for the condition of drainage is more complicated than the ordinary steady flow seepage problem, because the free surface is neither a flow line nor an equipotential line. The free surface must fulfil the condition that the intersections with the equipotential lines are all vertically equidistant. In addition, the free surface must be tangent to the upstream slope and must meet the slope of the impervious core at an angle ω which is dependent on the velocity v_β with which this point moves down the slope. This velocity must fulfil the conditions:

$$v_\beta = \frac{k \sin (\beta - \omega)}{w_y \cos \omega} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (65)$$

and

$$v_\beta = \frac{k}{100\, w_s \sin \beta} \left(\frac{D}{L}\right)^2 \frac{\partial U}{\partial T} \ldots\ldots\ldots\ldots\ldots\ldots (66)$$

Eq. 65 follows from considerations of the flow net at the point where the free surface meets the impervious boundary of the core, and Eq. 66 represents the rate of progress of drainage.

F. H. KELLOGG,[27] M. ASCE.—The discussions of this paper indicate several of the methods being used in the study of drainage without replacement from an external source. They should point the way toward a rational analysis of this type of ground-water flow which would lead to a much more intelligent use of local experience.

Mr. Cedergren outlines the method of attack by graphical flow nets. As pointed out at the close of Mr. Lambe's discussion, this completely neglects capillarity, which should not invalidate the analysis for pervious shoulders of dams, ground-water supplies, and similar conditions where capillary head is considerably less than the head inducing drainage. Just why the free surface of the water should be considered a source does not seem clear; the volume of water available there is infinitesimal. It appears more rational to consider the unsaturated boundaries of the drained body as a constant source through which air enters, the drains as constant sinks through which water escapes, and the free surface as a transient air-water boundary, as mentioned in the paper. Thus, distinction may be made between free draining materials, in which capillarity plays a minor role, and slow draining materials, in which capillarity plays a major role. The boundary between the two would depend on the ratio of effective capillary head to head inducing drainage, and would thus be variable, just as the boundary between turbulent and laminar flow, as defined by the Reynolds criterion, is variable.

Mr. Lambe gives an excellent resume of the mechanics of drainage. He questions the validity of Eq. 25*a*, doubtless because of lack of clarity in the basic paper. The writer hastens to add that he did not fail to plot this equation because he "* * * knew no reasonable way to compute the ultimate drainage quantity * * *"; in support, Fig. 26 is presented, plotting the computed curve, together with observed points for comparison. The test was made for standard Ottawa sand, so that any interested investigator may verify these results.

To clarify Eq. 25, it should be mentioned that it was derived for an initial condition of static saturation. The initial head distribution represents maximum potential head, L, minus maximum capillary head for equilibrium h_c. Because capillary channels are of a variety of sizes, and intercommunicate, the initial value of h_c was taken as varying from zero at the base to L at the top. As drainage proceeds, water flows from smaller into larger channels, accounting for increased net heads at the top of the sample. No flow of water into the sample is involved. An infinite hydraulic gradient occurs for an infinitesimal time at the beginning of drainage, making the volume of drainage during the

[27] Chairman, Dept. of Civ. Eng., Univ. of Mississippi, University, Miss.

first instant of flow indeterminate, not infinite. This analysis is similar to one for consolidation under its own weight of clay resting on sand.[28] As the initial head distribution for Mr. Lambe's "theory C" requires initial steady state flow, there is no comparison between this condition and that for which Eq. 25 was derived.

Mr. Riegel moves the discussion from the science of physics to the art of engineering. The importance of the modifying factors he has mentioned has been amply confirmed by field observations, including those of the writer.

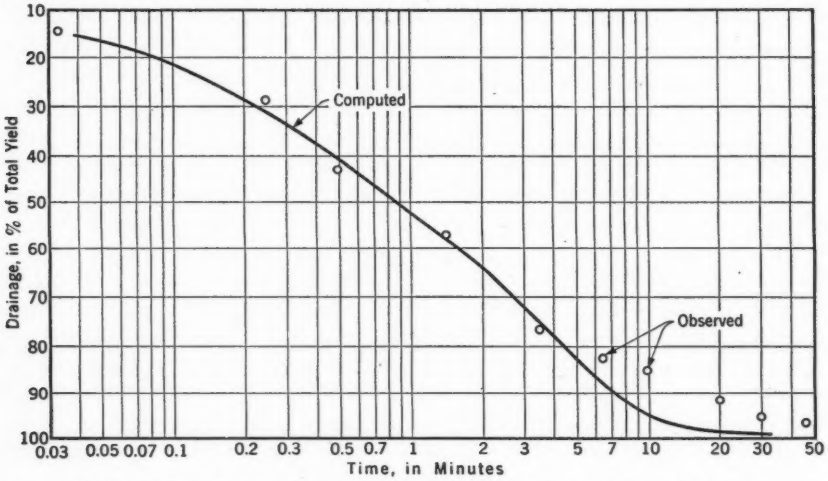

FIG. 26.—COMPARISON OF OBSERVED AND COMPUTED YIELDS FOR VERTICAL DRAINAGE

In fact, until sufficient field observations are available to serve as bases for working hypotheses, the usefulness of models and pure analysis is questioned. It is emphasized that the tests described in this paper were performed after field observations to secure a variety of checking conditions, and not to try to duplicate any field conditions.

Mr. Shannon presents an interesting description of drainage study by viscous fluid models. Although these are certainly a technical improvement on sand models, they do not give the heterogeneity of capillary channels leading to a true unsteady state condition in which every point of the body is a source. Like transient flow nets, they illustrate a steady state in which one boundary varies with time—air flowing steadily in at the top and fluid flowing out at the drain. This is not the condition described in the basic paper, nevertheless it is one that is extremely important, particularly in the field of ground-water supply, where the yield w_y, as assumed in Mr. Shannon's equations, is essentially constant. The viscous fluid models described in this discussion should have valuable potentialities under such conditions.

In conclusion, the author wishes to express his appreciation of the discussions submitted, which should be valuable additions to the scanty literature on unsteady state drainage.

[28] "Theorie der Setzung von Tonschichten," by K. Terzaghi and O. K. Fröhlich, F. Deuticke, Leipzig 1936, p. 115, section 57.

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DISCUSSIONS

MATRIX ANALYSIS OF PIN-CONNECTED STRUCTURES

Discussion

BY RUFUS OLDENBURGER

RUFUS OLDENBURGER,[6] ESQ.—The use of matrices in the treatment of pin-connected structures clearly demonstrates the linear equation theory underlying the solution for unknown forces and displacements at the joints. If the number of unknowns is excessive, the problem of inverting matrices is a serious one and might handicap the analyst.

The matrix γ_{AB} of Eq. 21 is a practical example of the use of the direct product of vectors, in this case the direct product of a vector such as $(X\ Y\ Z)$ by itself; that is,

$$\gamma_{AB} = (X\ Y\ Z) \times \begin{bmatrix} X \\ Y \\ Z \end{bmatrix} \ldots\ldots\ldots\ldots\ldots\ldots (23)$$

That α in Eq. 15 is a matrix with such direct products or sums of direct products as minors should simplify the treatment of α. The technique of setting up the matrix α for all components, and then dropping rows and columns corresponding to zero displacements is a convenient one.

The results of this paper are, of course, valid for first-order effects only, but this is generally sufficient. The assumptions underlying the paper are used in Eqs. 4 and 5. For Eqs. 4 Hooke's law is assumed; for Eqs. 5 it is supposed that the change in the angle θ in going from the unloaded to the loaded state of the bar is negligible. If x and y and u and v are the coordinates of the points A and B, respectively, so that the length of the bar is

$$L = \sqrt{(u-x)^2 + (v-y)^2} \ldots\ldots\ldots\ldots\ldots\ldots (24)$$

it follows that

$$dL = \frac{\partial L}{\partial u}\,du + \frac{\partial L}{\partial x}\,dx + \frac{\partial L}{\partial v}\,dv + \frac{\partial L}{\partial y}\,dy \ldots\ldots\ldots\ldots (25)$$

NOTE.—This paper by Pei-ping Chen was published in December, 1947, *Proceedings*

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in which

$$\frac{\partial L}{\partial u} = -\frac{\partial L}{\partial x} = \frac{u - x}{L} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (26a)$$

and

$$\frac{\partial L}{\partial v} = -\frac{\partial L}{\partial y} = \frac{v - y}{L} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (26b)$$

Assuming that the increment ΔL is the same as dL for practical purposes, Eqs. 5 are obtained from Eqs. 4.

When any one of the foregoing assumptions is no longer valid, the matrix method of approach breaks down.

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DISCUSSIONS

SHEARING STRESS DISTRIBUTION IN BOX GIRDERS WITH MULTIPLE WEBS

Discussion

BY JOHN E. GOLDBERG

JOHN E. GOLDBERG,[3] ASSOC. M. ASCE.—An interesting problem is presented in this paper. The author's statement that he has developed the principles of mechanics and the method which he has presented cannot be doubted. It is unfortunate, however, that the effort has been expended because not only current literature, but also an impressive list of textbooks[4,5,6,7,8,9,10] discuss and solve the problem in great detail.

The author presents, without derivation, only the simple case of a symmetrical two-cell box girder. This problem, as well as the much more general cases involving complete asymmetry, loads in any direction, the effects of taper, and greater numbers of cells, is practically routine in fields such as the aircraft industry. It has been handled so frequently during the past decade in this particular industry that many firms have developed standardized forms[7,9,10] to facilitate computations.

Certain limitations, not delineated by the author, should be mentioned. As presented in this paper the procedure is limited unnecessarily to symmetrical structures. The analysis as presented in the previously mentioned textbooks does not suffer from this restriction. Unless certain corrections are made, however, the general method is applicable under the following condi-

NOTE.—This paper by A. S. Neiman was published in February, 1948, *Proceedings*.

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[4] "Airplane Structures," by Alfred S. Niles and Joseph S. Newell, John Wiley & Sons, Inc., New York, N. Y., 1943, 3d Ed., Chapter XVI, p. 180.

[5] "Mechanics of Airplane Structures," by John E. Younger, McGraw-Hill Book Co., Inc., New York, N. Y., 1942, pp. 188-194.

[6] "Airplane Structural Analysis and Design," by Ernest E. Sechler and Louis G. Dunn, John Wiley & Sons, Inc., New York, N. Y., 1942, pp. 345-352.

[7] "Analysis and Design of Airplane Structures," by Elmer F. Bruhn, John S. Swift & Co., Cincinnati, Ohio, 1943, pp. A15.8–A15.14 and pp. C9.16–C9.18.

[8] "Strength of Materials," by S. Timoshenko, Pt. II, D. Van Nostrand Co., New York, N. Y., 1941, 2d Ed., pp. 278–282.

[9] "Design of Wood Aircraft Structures," *Bulletin No. 18*, Army-Navy-Commerce Committee on Aircraft Requirements, Govt. Printing Office, Washington, D. C., 1942, Chapter 3, Section 3.135.

[10] "Unit Method of Beam Analysis," by F. P. Cozzone, *Lockheed Paper No. 51*, Lockheed Aircraft Corp., Burbank, Calif., 1941.

tions only:

(*a*) When there are no restraints against warping at any cross section of box girder, or the method may be used only at sections remote from such restraints; and

(*b*) When the bulkheads, diaphragms, or ribs are assumed to be infinitely stiff in their own plane (but free to warp "out of plane"), or the method may be applied only at sections remote from the loads.

A word on the theoretical basis of the method of analysis may be in order, as it has not been included in the paper. The method is predicated upon the following:

(1) At every junction of two or more webs,

$$\Sigma q + \frac{V_x\, y\, \Delta a}{I_x} + \frac{V_y \times \Delta a}{I_y} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (8)$$

in which Σq is the algebraic sum of the shear flows in the webs approaching the junction; V_x and V_y are the applied shears perpendicular to the principal x-axis and y-axis, respectively; I_x and I_y are the moments of inertia of the bending material of the cross section about the x-axis and y-axis, respectively; and Δa is the elementary area of bending material located at the junction.

In the case where there is no bending element at the junction, Eq. 8 reduces to

$$\Sigma q = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (9)$$

The correctness of Eq. 9 is seen immediately (Fig. 5) if equilibrium in the z-direction is considered.

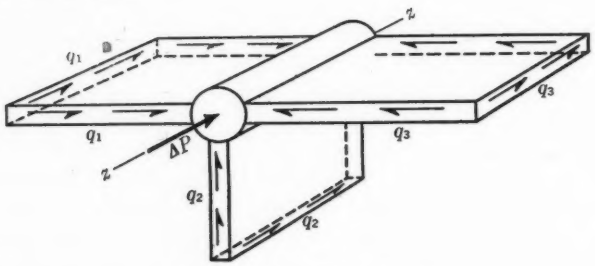

FIG. 5.—FORCES AT A JUNCTION

(2) The twist of an individual cell—

$$\theta = \frac{1}{2\,A\,G} \oint q \frac{ds}{t} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (10)$$

—is readily proved by strain-energy principles in the following manner:

At any point, the shearing strain is $\frac{q}{t\,G}$. Therefore, the total strain energy per unit length along the span is

$$U = \frac{1}{2\,G} \oint \frac{q^2\, ds}{t} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (11)$$

in which the symbol $\oint$ signifies the line integral around the periphery of the cell. If

$$q = q_0 + \frac{T}{2A} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12)$$

in which $\frac{T}{2A}$ represents the shear flow due to a real or virtual torque of magnitude T, and q_0 represents all other shear flows, then

$$U = \frac{1}{2G} \oint \frac{\left(q_0 + \frac{T}{2A}\right)^2 ds}{t} \quad \ldots\ldots\ldots\ldots\ldots\ldots (13)$$

and the twist per unit length along the span is

$$\theta = \frac{\partial U}{\partial T} = \frac{1}{2G} \oint \frac{2\left(q_0 + \frac{T}{2A}\right)\frac{1}{2A}\, ds}{t} = \frac{1}{2AG} \oint \frac{\left(q_0 + \frac{T}{2A}\right)}{t} ds \quad \ldots (14)$$

If T is not real, but virtual, Eq. 14 reduces to Eq. 10.

(3) The twists of all cells are equal, but only in the special case where the transverse bulkheads are infinitely stiff in their own plane. In many actual box girders, this restrictive condition is practically realized and the equivalence of cell twists becomes a valid assumption. (The correctness of this point may also be proved by use of the principle of least work.)

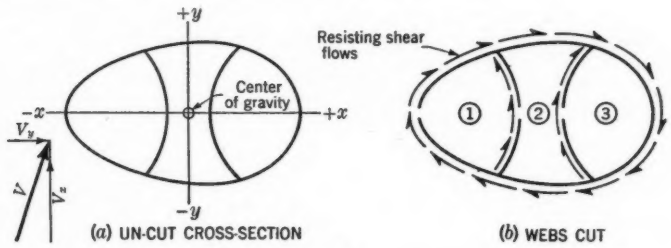

FIG. 6

With these points in mind, any cylindrical multiple-cell box girder may be analyzed. Taking, for example, a general asymmetrical girder, as shown in Fig. 6(*a*), the structure is cut back to a condition in which increments of shear due to bending elements may be evaluated (Fig. 6(*b*)) by cutting all but one web at convenient locations. A suggested procedure is as follows:

1. Locate the center of gravity of all bending elements, pass any two rectilinear axes through this point, and compute I_x, I_y, and I_{xy}, the moments of inertia about the x-axis, the y-axis, and the product of inertia about these axes, respectively. If the selected axes are the principal axes, then $I_{xy} = 0$.

2. Determine the distribution of static shear flows at every point in the cut-back structure which will be in equilibrium with the applied shear V, or its rectilinear components V_x and V_y. Note that this distribution, in conjunction with the external loads, will not, in general, satisfy the equilibrium condi-

tion $\Sigma T = 0$. Beginning with the shear flow equal to zero at each cut, the increments of shear flow due to bending elements may be obtained conveniently in the general case by the formula:

$$dq = \frac{V_y I_x - V_x I_{xy}}{I_x I_y - (I_{xy})^2} x\, da + \frac{V_x I_y - V_y I_{xy}}{I_x I_y - (I_{xy})^2} y\, da \quad \ldots\ldots\ldots (15)$$

3. Set up the equilibrium equation for torsion, selecting any convenient reference point about which the torsions may be calculated,

$$\Sigma T = 0 = T_o + T_S + q_1 2A_1 + q_2 2A_2 + q_3 2A_3 + \cdots + q_n 2A_n \ldots (16)$$

in which T_o denotes the torsional moment of the external loads about the selected reference point in the plane; T_S represents the moment of the resisting static shear flows determined in step 2; and $q_1, q_2, \cdots, q_n$ are the unknown closing shear flows that are to be added to the respective cells to satisfy the necessary conditions of continuity as well as equilibrium (Fig. 7).

It is pointed out, in connection with the equation of torsional equilibrium, that the moment of a shear flow of magnitude q lb per in., acting on a length of web or skin Δs inches in length, about any arbitrary reference point, O (Fig. 8), is

$$\Delta T = 2q\,\Delta A \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (17)$$

in which ΔA represents the area bounded by the element of skin and the two rays from point O to the two ends of the element.

FIG. 7.—CLOSING RESISTING SHEAR FLOWS

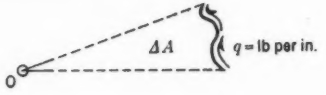

FIG. 8

4. Set up the continuity equations which, in the case of rigid bulkheads, are

$$\theta_1 = \theta_2 = \theta_3 = \theta_n \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (18)$$

using Eq. 10

5. Solve the equations of continuity simultaneously with the equilibrium equation to obtain the closing shear flows, $q_1, q_2, \cdots, q_n$.

6. Obtain the net shear flows, which at every point are the static shear flow plus the proper closing shear flows. Note that in webs common to two cells the two closing shear flows act in opposite direction if they are of the same sign.

Corrections for Transactions: In February, 1948, *Proceedings*, on page 163, change the line preceding Eq. 4*a* to read: "Hence the following formulas for the shear strain are obtained:"; on page 164, the text containing Eqs. 7 should read "Therefore, A_1 times the left side of Eq. 4*a*, step 3, must be equal to A_2 times the left side of Eq. 4*b*; thus:

$$A_1 F(\mathrm{I}) = A_2 F(\mathrm{II}) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)"$$

On page 164, delete Eq. 7*b* and in the second line following Eq. 7, delete the phrase "with the result that"; and, in Table 1, Col. 7, add, for member A(D), "27.93" and for member F(G), "24.0."